DISCUSSION, PROCEEDINGS V. 56

Discussion of papers published in the July through September, 1959, JOURNALS appears in the concluding pages of this March issue, as the Institute continues its quarterly publication of discussion. Papers published October through December, 1959, will be discussed in the June 1960 issue; discussion of papers published January through March, 1960, will be published in Part 2, September 1960 ACI JOURNAL. Discussion of April through June papers will appear in Part 2, December 1960 along with index and errata for V. 56.

The material explained
above will be found
following issue no. 12

Discussion of a report by ACI Committee 605:

Recommended Practice for Hot Weather Concreting (ACI 605-59)*

By ROMAN MALINOWSKI and COMMITTEE

By ROMAN MALINOWSKI†

The recommended practice for hot weather concreting fills a gap in an important field of building construction which is of particular value for hot climate countries.

We submit here a few remarks to the discussion on this subject.

Definition of hot weather concreting — A definition for hot weather concreting is lacking in the report, as is the description of the influence of factors such as temperature, moisture, and wind on the various stages of concreting and curing. The mutual dependence of temperature and moisture should be stressed here. It seems that it would be helpful to introduce a classification of normal, aggravated, and critical conditions of hot weather concreting and a definition of their borderline values, together with instructions for operation in each of these conditions.

Influence of high temperature and moisture on the strength of concrete — It is doubtful whether concrete loses strength at high temperatures, as stated in the report. It is my opinion that with suitable moisture control the strength of concrete is not decreased and in some cases may even increase. It is a known fact that greater than average strength can be obtained at high temperatures by using low calorific cements, e.g., slag and blast furnace cements or pozzolanic cement.

The report does not indicate the beneficial influence that hot weather has on shortening the time of curing which subsequently effects the quick removal of the formwork from the concrete.

Concrete testing—The question of storing concrete test specimens in hot weather is also important. It should be stressed that the specimens should be stored under conditions similar to those of the curing concrete from which they had been taken.

Further remarks — When discussing the influence of hot weather on strength and setting of concrete, the report omits the question of durability.

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Neither does the report consider the thickness of the concrete, nor does it deal with the permissible length of time between mixing and placing of the concrete during various conditions of hot weather.

COMMITTEE CLOSURE

Committee 605 has reviewed with much interest and appreciation Professor Malinowski's discussion. The following comments deal with the items of that discussion in the order presented.

Definitions — The term "hot weather" may mean different things depending on conditions for the particular job and location, with the result that a categorical definition might be misleading. For example, a temperature of 80 F might be "hot" for a flat slab exposed to the sun on a dry day, while 90 F would not be excessive under more favorable conditions of surface area, humidity, and degree of exposure. It seems that the committee's general coverage of the subject, along with the indications of what constitute critical conditions within the concrete itself, provide a sound basis for the recommendations in the report. It is true that certain detailed relationships mentioned by Professor Malinowski have not been discussed, but information on these is available in the references cited. The committee felt it should study the data and make specific suggestions rather than offer a lengthy review of the readily available literature.

Influence of temperature and moisture — Professor Malinowski apparently disagrees with the committee's interpretation of the rather large amount of literature indicating that high temperatures of mixing and curing, even when adequate moisture is provided, are harmful to long-range strength development. A number of specific references are given in the report, but it may be worth while to mention a couple which reflect the consensus.

The *Concrete Manual* of the Bureau of Reclamation states: "Limitations on the maximum temperature of concrete and on the placing of concrete during hot weather have been imposed because of the impairment of quality and durability resulting when concrete is mixed, placed, and cured at high temperatures. This impairment affects several different properties of the concrete: first, the ultimate strength of concrete that is mixed and cured at high temperatures is never as great as that of concrete mixed and cured at temperatures below 70 F . . ."

Extensive researches by the Portland Cement Association led to the following conclusions reported in the paper, "Effect of Mixing and Curing Temperature on Concrete Strength," by Paul Klieger in the ACI JOURNAL, V. 29, No. 12, June 1958 (*Proceedings*, V. 54), pp. 1063-1082:

"(2) At 1, 3, and 7 days, concrete strengths increase with an increase in the initial and curing temperature of the concrete.

"(3) Increasing the initial and curing temperatures results in considerably lower strengths at 3 months and 1 year.

"(5) Strengths of concretes made with the three cement types used in this study were influenced in a like manner by temperature; differences were in degree only.

"(6) These tests indicate that there is a temperature during the early life of the concrete which may be considered optimum with regard to strength at later ages, or more strictly, at comparable degrees of hydration. This temperature is influenced somewhat by cement type. For Types I and II this temperature is 55 F, for Type III it is 40 F."

While there is little documentation either to support or to contradict Professor Malinowski's contention that "low calorific" cements are benefitted by high temperature, it is interesting that Klieger concluded that Type II, as well as Types I and III, was harmed. Differences among the types were in degree only. Data in *NRMCA Publication* No. 53 (Reference 14 of the report) indicate that, for Type I-S cement, concrete cured 28 days in 100 F water was 17 percent weaker than the same concrete moist-cured at 73 F. On the other hand, parallel tests of concrete with fly ash showed an increase of 10 percent in 28-day strength resulting from the high temperature. It appears that the committee would not be justified in generalizing with regard to the effects of high temperature on the low calorific cements.

Testing — The question of storage of concrete test specimens can lead to serious misunderstanding unless the function of the tests is clearly understood. The evaluation of concrete quality and determination of compliance with specifications must be based on tests made in accordance with standard methods of fabrication, protection, curing, and testing. This is recognized in the committee's report and in the requirements of responsible specification-writing agencies. The report also acknowledges the potential usefulness of *properly interpreted* tests of job-cured specimens for judging in a highly approximate way the condition of the concrete in the structure.

Further remarks — It is true, perhaps, that the report does not cover in as much detail as might be desirable the questions of durability, massiveness of section, and length of mixing time, although the latter is discussed in some detail in the section on production and delivery. Constructive suggestions relating to durability and size of section would be welcomed by the committee.

Discussion of a paper by Phil M. Ferguson and Farid N. Matloob:

Effect of Bar Cutoff on Bond and Shear Strength of Reinforced Concrete Beams*

By GEOFFREY BROCK, BRUCE H. FALCONER, R. S. SANDHU, JOSEPH TAUB and A. M. NEVILLE, and AUTHORS

By GEOFFREY BROCK†

The information presented in this paper should be of considerable interest to all reinforced concrete engineers. It suggests that a beam with conventionally curtailed reinforcement may have a smaller ultimate load capacity than a similar beam having all reinforcement carried to the supports.

When examining the results obtained from beams tested to destruction, the writer finds it illuminating to ascertain the dependence of an ultimate load or moment on the amount of longitudinal reinforcement. Knowing how the ultimate flexural capacity depends on the proportion of reinforcement, it is possible to gage, from any deficiency in the observed moment capacity, how far the effects of shear, bond or anchorage have been responsible for premature failure. In this case the authors' test results have been used to produce Fig. A, in which the ultimate moment at the load points, expressed in terms of $f_c'bd^2$, is plotted against the midspan reinforcement index, p/p_o.

In the absence of complete dimensions for each specimen, it has not been possible to include the "continuous" beam tests in this analysis. However, it is noted that most of these, the beams of Group VIII, were over-reinforced, in contrast with the other groups. For over-reinforced beams the strength of the concrete, rather than deficiencies in the arrangement of the reinforcement, would govern collapse, so it may not be certain that the improved behavior of this group can be attributed to the stirrups.

Reverting to Fig. A, it will be seen that all the results which lie close to the Whitney flexural line represent beams with bent-up bars or full-length reinforcement. The other arrangements gave results lying between the full flexural capacity and 60 percent of it. Bearing in mind that the minimum shear-span:depth ratio was 4.4, one would expect beams with full-length reinforcement to attain their full flexural

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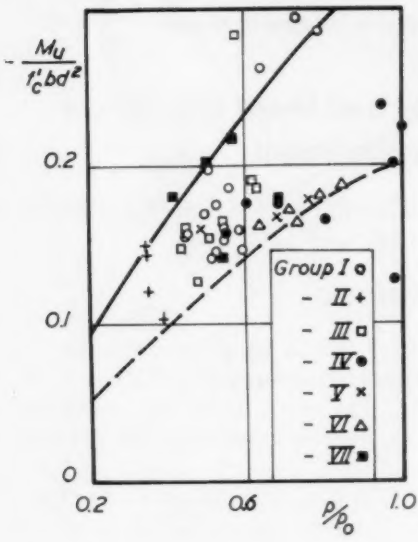

Fig. A—Ultimate moment of resistance related to midspan reinforcement index (Based on authors' tests). Full curve shows values for flexural failures calculated by Whitney's method, broken curve shows 0.6 of these values

strength and the tests confirm this. The marked deficiency of so many of the other specimens would seem to be caused by inadequate anchorage rather than excessive shear. A similar decrease in strength occurred in some tests made by the writer when the overhang beyond the supports of simple beams, with full-length reinforcement, was reduced. This had the same effect as curtailing the anchorage.

The general remarks at the beginning of the paper, commenting on the lack of tests and knowledge of this kind and the necessity for realistic test specimens, seem most timely. Too many of the rules about shear, bond, and anchorage are based on a limited number of tests on unusually proportioned specimens. With more information of the kind given by the authors it may be possible to get better design methods and even simplify detailing.

By BRUCE H. FALCONER*

Fig. 9, showing test results of Group VI beams, suggests that the effect of stopping off plain bars may be less severe than that of stopping off deformed bars. However the lowest strength of the group occurred with all bars plain. Were the plain bars hooked?

To minimize the local influence of transfer of bond tractions from cut to continuous bars, may it not be preferable to bend bars up at less than 45 deg to the beam axis?

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For continuous beams subject to racking and possible yield in earthquake, the discusser on occasions bends down half the top negative steel, at between 10 to 20 deg to the beam axis, from points just clear of the supports. This bent steel is generally terminated without reverse bends or hooks, at about 4 in. of bottom cover. Stirrups are determined at the ends of the bent bars and, provided all considerations allow, the spacing of stirrups is kept constant back to the supports.

By R. S. SANDHU*

The authors have to be congratulated on a welcome study. It appears, however, that the complication is not so much on account of the tension in the cutoff bar that is transferred to the continuing reinforcement as due to the manner in which it is transferred. Continuing bars are strained, along with the concrete encasing them, in developing tensile stresses. Variation in this tensile stress along the length of reinforcement is associated with bond stresses. Longitudinal strains at a particular depth in a concrete beam being uniform, all bars share, more or less equally, the tensile force in steel. Now in cases where the cutoff bar is bent up, it is raised to an area of reducing longitudinal strains and beyond some stage ceases to share in the longitudinal tensions. Reduction of the longitudinal stress in such a bar would be fairly gradual. In case of a bar cut off without being bent up, however, the cutoff bar fully shares in the longitudinal tensions up to a point close to its end and, as the tension has to reduce to nil at its termination, the reduction takes place in a short length of bar and limiting bond stresses are set up. At the same time the continuing bars are suddenly loaded more heavily. This would lead to heavy local complications in the stresses. This would seem to hold to some extent even for cases in which web reinforcement is provided although cracking would naturally be arrested by web reinforcement.

By J. TAUB† and A. M. NEVILLE‡

The tests by the authors showed interesting results. Cutting off bars in the tension of the beams reduced their shear strength to a great degree. This might be explained as follows. There is a sudden change in strain in concrete at the cutoff point, and as a result of this a crack forms at that point. Concrete is sensitive to sudden changes in strains. From the tests by the authors it is seen that the shear strength of a beam decreases as the area of steel cut off at one point increases. In this case the difference in concrete strains at the cutoff point is greater,

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the diagonal crack at this point develops quicker, and the shear strength of such a beam is lower. The same applies to bending up bars. Many tests on beams which were provided with bent-up bars showed that a diagonal crack formed at the section where the inclined bar was bent up. The tests by the authors showed the same result. The points of bending up bars should be, therefore, staggered.

Beams W3 and M7 tested by the authors had two # 4 bars cut off at the inner section and one # 4 bar at the outer section. As the area of cutoff bars at the inner section was greater than that at the outer section the beams failed at the inner section.

The test results of Beams M9, M3a, and M2a, will now be discussed. In the first two beams two # 4 bars continued up to the support and the other two # 4 bars were cut off at a distance of 5.25 in. from the reaction. The average value of v_{3700} for these two beams was 149 psi. Beam M2a was reinforced with four # 4 full length bars. The shear strength of this last beam was 33 percent greater than that of the first ones.

Moersch* reported on similar tests performed on T-beams. The beams were reinforced with plain round bars and provided with closely spaced stirrups. Two beams were tested. In one of the beams all the main reinforcement continued up to the support, in the other one a part of the tension steel was cut off in accordance with the diminution of the bending moment. The shear strength of the first beam was 36 percent greater than that of the second one. From that it is seen that the use of stirrups does not prevent the ill effect of cutting off bars in the tension zone of a beam. A detailed description of above tests is included in our paper on shear resistance of reinforced concrete beams which has been submitted to the ACI JOURNAL.

Special attention was given by the authors to investigating the effect of the length L'' from the reaction to the last cutoff point on the shear strength. The test results showed that the shear strength of the beam decreases as L'' increases. The reason for this seems to be as follows. After formation of a diagonal tension crack and redistribution of internal forces the shear strength of a beam depends on the resistance of the compression zone at the upper end of diagonal crack and of the tension zone at its lower end. With increasing value of L'' the stresses caused by the bending moment also increase at the respective section and the shear strength of the beam decreases accordingly.

The effect of bent-up bars on the shear strength of a beam will now be discussed. In Fig. B, three beams of Group I are represented and for comparison Beam M2a, with full length straight bars and no web reinforcement, is added. The main cracks which caused the beam failure, as described by the authors, are shown.

*Moersch, E., *Der Eisenbetonbau*, Band 1, Haelfte 2, Verlag von K. Wittwer, Stuttgart, 1929.

The values of v_{3700} for the beams varied from 174 to 247 psi. The authors seem to attribute the differences in shear strength of these beams to differing values of f_y. In the writers' opinion the value of f_y is a decisive factor in the case of flexural failure in tension, but not so in the case of shear failure. From Fig. B it is seen that in Beams S1 and S3 the diagonal tension crack was crossed by two bent-up bars, in Beam S4, however, by one only. It can be assumed, therefore, that in the first two beams the crack opened less than in the third one. This seems to be the reason that the shear strength of the first two beams was so high. The somewhat lower shear strength of Beam S1, as compared with Beam S3, seems to be the result of greater value of L″ in this beam. The shear strength of Beam S4 was even lower than that of Beam M2a with no web reinforcement.

From the tests by the authors it is clearly seen that bending up bars gives much better results than cutting them off. It seems, however, that bending up bars alone does not protect a reinforced concrete beam from the action of diagonal tension. The spacing of bent-up bars or the arrangement of the web reinforcement is the decisive factor.

The exploratory tests by the authors on "continuous" beams showed interesting results. But on the basis of data presented by the authors it is somewhat difficult to get a full picture of behavior of the beams. It seems probable that investigators would appreciate the inclusion of additional information in the authors' closure concerning the beam span, the length of the overhang, and the crack pattern, for instance for Beam Y1 as compared with the corresponding simple span Beam Y4.

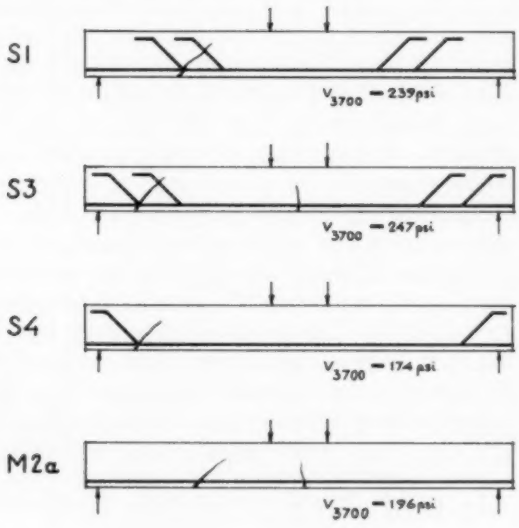

Fig. B—Comparison of shear strength of beams with various arrangements of web reinforcement

The tests by the authors are of great importance in building practice. The authors rightly pointed out that it is hazardous to cut off bars in a tension zone. In the writers' opinion one of the main reasons of the beam collapse in the warehouse at Shelby was the accumulation of the cutoff points within a short length of the beam.

AUTHORS' CLOSURE by PHIL M. FERGUSON*

That discussion of this paper has come from four countries in three other continents is most gratifying. Possibly because the practice of cutting off tension bars in a tension zone is less frequently permitted outside of the United States, there was no dissent from the major conclusion of the paper that bar cutoff can be a source of serious weakness in beams not having web reinforcement.

Mr. Sandhu suggests that local stress complications would also seem to exist when stirrups are present. Dr. Taub and Dr. Neville report tests of two T-beams with plain bars by Moersch in which the use of stirrups did not eliminate the ill effect produced by bar cutoff. Since the original paper was prepared, the writer has tested 35 additional "continuous" type Y-beams, using larger bars and heavier beams and attempting to avoid diagonal tension failures or complications. Unless extreme precautions are taken, he has found that stirrups are not fully effective in minimizing diagonal cracking at cutoff points. As a result of some unsatisfactory experiences he now specifies for these beams: (1) the initial stirrup located not more than 1/2 to 3/4 in. from the bar cutoff; (2) the stirrup spacing not over 1.5 in., for a beam with an effective depth of over 7 in.; and (3) stirrups of closed type extending within 1/2 in. of the compression face of the beam. It does not appear that the stirrup size itself is very critical. The difficulty has been in preventing the diagonal crack (always starting with a moment crack near the cutoff point) from finding some path which could almost avoid intersection with the stirrups. These necessary precautions indicate that the mere presence of stirrups, as ordinarily designed, may be somewhat ineffective at bar cutoff points.

Mr. Brock has added a significant comparison in the form of a graph showing relative moment resistance for various degrees of reinforcement. The addition of the Y-beams of Group VIII, for which he had inadequate data, would not have noticeably changed his presentation. As he notes, this group was heavily reinforced. However, all except Beam Y4 carried substantial bottom steel which was in compression at the critical sections. Only the simple span Beam Y4 approached its moment value in compression, reaching about 92 percent of the calculated ultimate. This beam failed in diagonal tension insofar as visual inspec-

*It is regretted that it was not feasible to consult with the coauthor, Mr. Matloob, in preparing this closing discussion.

tion would indicate, with the diagonal crack developing gradually from a moment crack about half way between the reaction and the cutoff point. Final failure included "shearing" off a long thin slab at the top of the beam over the stirrups and cracking horizontally at the bottom along the steel to the end of the beam.

Messrs. Taub and Neville asked for details and a comparison of the crack pattern of "continuous" Beam Y1 which failed by splitting and the simple Beam Y4 which has just been described. Beam Y1 developed a small diagonal crack on the end of a moment crack which started 2 in. beyond the cutoff point of adjacent bars. Although this diagonal crack formed at about half the ultimate load, the stirrups controlled it completely. A central top splitting crack over the L'' development length of the bar started developing at two-thirds of the ultimate load. At 90 percent of ultimate load this split had developed over 60 percent of the 18 in. L'' length in spite of two shallow intersecting moment cracks which had developed. At the same time a side split had developed for 6 in. along the bar, extending from the diagonal crack. The top splitting crack finally developed to the end of the bar, and the bar thus freed broke off wedges of concrete some 20 in. long along each side of the beam, the side cracks having a slope of about 1 in 5 back to the diagonal crack. The failure load was on the beam about 5 min before failure occurred, thus affording an ample opportunity to observe the way in which failure was developing.

The above description indicates that phenomena at the support where moment was maximum played no part in the failures. Consequently the overhanging cantilever was varied as convenient, but always such that the "continuous" beams each had 38 in. between point of inflection and support. Bottom (compression) steel was two #6 bars in all cases except in simple span Beam Y4 where it was only two #2 bars to support the stirrups. Location of compression steel was only roughly controlled, around 1-in. clear cover.

The explanations of Messrs. Taub and Neville are logical but some modifications of Fig. B are desirable to match failure patterns actually observed. The failure crack for Beam S1 developed about midway between reaction and the outer bend-up point. The top of the crack crossed this outer bent bar near its upper end and went *over* the other bent bar. In beam S3 a diagonal crack (somewhat steeper than shown) developed considerably nearer the inner bend-up point. This crack had no part in the failure which developed suddenly and completely from just inside the inner bend-up point up to the load point. The tension steel was beyond its yield point and such diagonal failures often occur before the moment failure is complete. The failure of Beam M2a was almost the same and occurred under similar yielding of the steel. Beam S4 had a straight bar cutoff point midway between reaction and load point and a

moment crack there gradually developed into the typical diagonal tension failure.

These discussers assign the reduced strength of beams with large L'' to the increased compressive zone stresses existing in such cases. This is undoubtedly a factor, but observation of the tests leads to a further factor or a different aspect of the same factor. A small L'' value meant that flexural cracking was much delayed because of the smaller moment acting at the cutoff point. This delayed (and shallower) cracking seemed to be a favorable factor.

Mr. Falconer asked some specific questions. The plain bars used in a few specimens were not hooked. Presumably smooth bars at a cutoff point lose their stress more gradually, which is favorable, but their use as the full length bars leaves a basic weakness in bond strength which gives low over-all strengths.

Bars bent at 45-deg slope performed well in these tests, but a flatter slope would certainly seem to be no weaker and for large size bars might be stronger. The suggestion that some bars over the support be bent down just clear of the supports at an angle of 10 to 20 deg sounds good from the standpoint of shear strength. However, the writer would be skeptical about whether such bars would have enough length in the top of the beam to develop their tension yield stress before moment failure might develop.

The writer wishes to thank each of the discussers for their contributions.

Discussion of a paper by M. M. Lemcoe and C. H. Mahla:

Prestressed Overlay Slab for San Antonio Airport*

By F. M. MELLINGER and AUTHORS

By F. M. MELLINGER†

The authors have presented the design method of a prestressed concrete overlay pavement that was constructed about 3½ years ago. Since that time considerably more information has become available on prestressed pavements. Some of this information is summarized in the Highway Research Board *Bulletin* No. 179 entitled "Prestressed Concrete Pavement Research," and in the references listed for this discussion. Probably most pertinent to the discussion are the results of traffic tests on the Sharonville prestressed overlay given in Reference 6.

The method of design, or rather the evaluation of the prestressed overlay given in the paper is unduly conservative, since it allows no cracking in the bottom of the overlay or base pavement. Normally, prestressed pavements are designed on the basis of the maximum negative moment at the top of the slab, rather than the maximum positive moment in the base of the slab as for plain concrete pavements. The procedure outlined in Reference 7 permits the formation of tension cracks in the bottom of the pavement under load.

Applying the method of design given in the paper to the Sharonville prestressed overlay pavement, a factor of safety of less than one is obtained for a 100,000-lb twin wheel gear loading. The Sharonville overlay has a minimum thickness of 4 in. and averages 4.4 in. thick. It is separated from the 6-in. base pavement of plain concrete by waterproofed kraft paper. The subgrade is a fat clay having a modulus of about 65 lb per cu in. No leveling course was necessary over the base pavement. Table A summarizes the physical properties of the concrete in the overlay and base pavement.

TABLE A—SUMMARY OF PHYSICAL PROPERTIES OF CONCRETE, SHARONVILLE OVERLAY

Property	Pavement	
	Base	Overlay
Flexural strength, psi	775	725
Compressive strength, psi	5000	4500
Modulus of elasticity, psi	4.3×10^6	4.4×10^6
Poisson's ratio	0.20	0.20

The full 6 in. for the base pavement was used in the analysis. The

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gear loading applied was 100,000 lb on twin wheels spaced on 37.5-in. centers, with elliptical tire contact areas of 267 sq. in. The elliptical tire print shape was used rather than the Portland Cement Association procedure. The PCA procedure would give a slightly greater stress.

In comparing the Sharonville overlay with those at San Antonio, the initial prestress before losses was approximately 400 psi, as compared to 425 and 175 psi. The subgrade modulus was 65 lb per cu in. at Sharonville, as compared to 171 lb per cu in. at San Antonio, while the concrete properties were of the same general order. There were cracks in the 6-in. base slab at Sharonville also. The asphalt leveling course at San Antonio should add some strength to the overlay.

Even though the factor of safety for the Sharonville overlay computed by the method given in the paper was less than one, it sustained 2068 repetitions of the 100,000-lb twin wheel loading before failing. This failure occurred over an expansion joint in the base pavement. For a single load application with an 18-in. diameter plate over a construction joint in the base pavement the load required to cause failure at the prestressed overlay was 225,000 lb. This is an indication, that for the design of prestressed pavements, the assumption of tension cracks forming under load in the bottom of the pavement is reasonable. These cracks do not appear on the surface and are closed by the prestress when the load is released. When they occur, there is merely a redistribution of stress in the pavement due to the formation of a plastic hinge at the cracks.

There is no reason why the method of analysis set up in the paper could not be used to compute the negative moment at the top of the prestressed pavement, with appropriate allowances made for warping or temperature stresses and the effects of load repetition. However, some clarification on the method of selecting the modulus of elasticity for the subgrade soils and the limiting deflection would be desirable.

In attempting to check the factors of safety given in the paper for the overlays, they appear to be 1.08 and 1.37 rather than 1.1 and 1.7. The values of stress, prestress, and flexural strength given in the paper were used; however, there may have been some difference in interpretation or a larger value for the flexural strength of the concrete applied.

On the basis of the performance of the Sharonville overlay under traffic loading, it would appear that the overlay at San Antonio with the lower order of prestress should carry DC-6 traffic and heavier for a number of years without showing distress. An important factor in this consideration is the greater degree of subgrade support at San Antonio, as compared to that at Sharonville. This should have a greater effect than a theoretical comparison would indicate, since prestressed pavements will tolerate much larger deflections than plain concrete pave-

ments. The most critical area in the San Antonio overlays are the edges adjacent to the transition course. This is the area where pavement distress under traffic loading will probably appear first.

The authors of the paper have presented a method of analysis that may be applied to both prestress overlays as well as plain concrete overlays. However, at the present time it should be used with caution, until more experience in its application is available.

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7. Molke, Eric C., "Prestressed Concrete Pavements," *Proceedings,* ASCE, V. 85, AT 3, July 1959.

8. Carlton, Paul F., and Behrmann, Ruth M., "Model Studies of Prestressed Pavements for Airfields," *Bulletin* No. 179, Highway Research Board, 1958, pp. 32-50.

AUTHORS' CLOSURE

Mr. Mellinger has made a most interesting comparison on the design aspects and performances of the San Antonio and Sharonville overlays. Mr. Mellinger states the method of design of the San Antonio overlay was unduly conservative in light of present knowledge on the behavior of prestressed overlays. Undoubtedly, there is now more test data and experience to justify a design procedure which tolerates the formation of tension cracks in the bottom of the overlay under load, without damage or premature fatigue failure. The reason that cracking was not permitted in the San Antonio overlay is that there was concern that any such cracking which would occur in the relatively thin layer of concrete directly under the rigid metal conduit might propagate around the conduit to the top of the overlay itself. If this occurred crack patterns conforming to the geometry of the conduit could develop throughout the slab seriously affecting the service performance of the slab. Had small individual wires, rather than groups of wire in rigid conduit been used, resulting in considerably more concrete cover top and bottom, then cracking on the bottom of the overlay may have been considered permissible.

Mr. Mellinger mentions that the authors' method of design, when applied to the Sharonville overlay, results in a factor of safety less than one, under a 100,000-lb wheel load, and further states that 2068 repetitions of the 100,000-lb wheel load were sustained before failure. The San Antonio overlay is subject to approximately 400 taxi runs daily, the greater number being large commercial transport aircraft. It would thus have accumulated the 2068 repetitions in slightly more than 5 days. Any overlay having a fatigue life of only 5 days would, indeed, be unsatis-

factory and it is perhaps not surprising that the authors' method gave a factor of safety less than one when applied to the Sharonville overlay.

The factors of safety of 1.1 and 1.7, rounded off to one digit after the decimal point, were arrived at with the formula generally used by pavement engineers for unprestressed pavements; namely

$$\text{Factor of Safety} = \frac{\text{Modulus of Rupture}}{\text{Tensile Stress}}$$

the tensile stress in our case being the theoretical tensile stress in the bottom of the overlay minus the effective prestress. The calculations are as follows:

$$\text{F.S. (weaker slab)} = \frac{700}{738 - [(175)\ (0.85) - 50]} = 1.1$$

$$\text{F.S. (stronger slab)} = \frac{700}{738 - [(425)\ (0.85) - 50]} = 1.7$$

Mr. Mellinger arrived at his factors of safety of 1.08 and 1.37 by modifying the modulus of rupture of the concrete, rather than by reducing the theoretical stress in the amount of the effective prestress. Thus,

$$\text{F.S. (stronger slab)} = \frac{(425)\ (0.85) - 50 + 700}{738} = 1.37$$

It is the authors' opinion that modifying the theoretical tensile stress appears to be somewhat more logical than modifying the modulus of rupture of the concrete which is an intrinsic materials property. If Mr. Mellinger had applied the formula used by the authors on the Sharonville overlay, perhaps he would have gotten a factor of safety greater than unity.

Finally, it is not obvious to the authors that the asphalt leveling course adds to the strength of the system, since any gain in increased over-all thickness of the system might more than be offset by the increased stress due to the relatively low stiffness ratio of the asphalt to the concrete.

Discussion of a paper by James O. Henrie:

Properties of Nuclear Shielding Concrete*

By D. CAMPBELL-ALLEN and C. P. THORNE, ROMAN MALINOWSKI, and AUTHOR

By D. CAMPBELL-ALLEN† and C. P. THORNE‡

Thanks are due to the author for his useful presentation of data relating to the construction of actual shields. It is interesting to read that it has proved worthwhile to use boron additive in both Mix 1 and Mix 2, which are intended primarily as neutron shields, even though the main aggregate in both mixes is an iron ore and in the case of Mix 2 iron scrap has been added. Presumably the economic advantage is primarily that of reduced shield thickness, with the advantages that accrue from this. In this connection it is worth noting that the addition of boron to increase the attenuation in a concrete shield is limited by the temperature rise that can be sustained by the shield, and unless it can be shown that higher temperature rises can be accepted without inducing dangerous thermal stresses or reduced strengths, then the more effective attenuation that boron produces is at present of little value. With higher temperatures, too, drying of the concrete will proceed at a rapid rate. The resultant changes in relaxation lengths both for neutrons and gammas, and changes in conductivity and specific heat lead to further substantial increases in temperature.

From work now in progress in the University of Sydney, it is clear that values for thermal conductivity vary widely with the moisture content of the concrete, and the general figures quoted by the author are so rough as to be of little value. For an ordinary concrete, with the composition given in Table A, made with a dolerite aggregate, it has been found that the thermal conductivity ranges from 0.7 Btu per hr per sq ft per F per ft when the free moisture content of the concrete is zero, to 1.1 when the free moisture content is 20 percent by volume, with a closely linear variation between these extremes. Figures obtained in the same apparatus for concretes made with heavy ores are:

*ACI JOURNAL, V. 31, No. 1, July 1959 (*Proceedings* V. 56), p. 37. Disc. 56-6 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).

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‡Australian Atomic Energy Commission Research Student, University of Sydney, N. S. W., Australia.

Aggregate		Thermal conductivity, Btu per hr per sq ft per F per ft
Hematite	Wet	2.42
	Dry	1.65
Barytes	Wet	1.10
	Dry	0.70

The moisture content is believed to be a most important variable in assessing the behavior of a concrete shield, particularly when operation at higher temperatures than at present common is contemplated. This aspect of shield design is one receiving attention by the writers at the present time.

TABLE A

Aggregate type	Dolerite	Hematite	Barytes
Coarse aggregate,* lb per cu yd	1984	3210	3100
Fine aggregate, lb per cu yd	1190	1190	1190
Cement, lb per cu yd	512	512	512
Water, lb per cu yd	342	342	342
Saturated density, lb per cu yd	146	193	189
Free moisture, percent	18	21.6†	18

*All mixes were of the same proportions by volume for coarse aggregate: fine aggregate:cement:water; the same fine aggregate and cement were used for all mixes. Fine aggregate was Nepean sand. Cement was normal portland cement. Heavy coarse aggregates were regarded to give the same grading as the dolerite.

†The hematite absorbed 3½ percent by weight of free moisture.

The continued specification of strength as a requirement for shielding concrete may be seriously misleading, since in cases where thermal stresses predominate the need is for concrete with a low value of Young's modulus and a large capacity for tensile strain. These features accompany concrete of low strength and the possibility might therefore be considered of specifying a maximum strength in appropriate conditions.

By ROMAN MALINOWSKI‡

The author discusses the problem of biological shielding against neutron radiation and against combined radiation of gamma rays and neutrons. He deals in detail with the function of boron additives in capturing thermalized neutrons and he describes the influence of these additives on the setting of cement. He indicates the possibility of regulating this process by the addition of calcium chloride. The aggregate he discusses consists of crushed stone and sand of magnetite or hematite.

The author considers the prolongation of the setting time, caused by the addition of boron salts, as harmful. This phenomenon however is generally regarded as useful in mass concrete because it is usually accompanied by a lower hydration temperature and thus the maximum temperature is delayed. This also facilitates the continuity of the placing of concrete.

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Measurements of the temperature of hydration carried out by the writer on pastes of various cements showed that the addition of borax prolonged the setting time 2-3 times and considerably lowered the temperature of hydration, for instance: when using cement similar to Type I, the temperature was lowered from 70 C to 35 C and the maximum was reached after 24 hr instead of 10 hr at an external temperature of 27 C.

Almost all properties of ordinary fresh and hardened concrete depend on the quantity and density of the cement paste. In heavy concrete the aggregates and the cement paste also ensure the shielding against radiation. The aggregate captures gamma radiation and the cement paste, which contains hydrogen and boron additives, captures the neutrons. A characteristic of the heavy concrete is a considerable difference between the volume weight of the aggregate and the paste. Ordinary aggregate is about 1.5-2 times heavier than the cement paste. Heavy aggregate is 3 or more times heavier. This can be availed of when calculating and executing trial mixes.

Calculating the ingredients of concrete of a fixed volume weight

An *a priori* fixed volume weight of concrete is an additional condition in calculating its ingredients. The volume weight of concrete is in such a case the sum of the weights of the paste and aggregate. From the formula of density we obtain:

$$G_B \cdot 1000 = G_P \cdot V_P + G_A \cdot V_A \quad \text{in kg per cu m} \qquad (1)$$

where G_B, G_P, and G_A are the volume weights of the concrete, paste, and aggregate and V_P and V_A are the volume of the paste and aggregate. As

$$V_P = 1000 - V_A \quad \text{in liter per cu m}$$

a transformation of Eq. (1) gives:

$$V_P = \frac{G_A - G_B}{G_A - G_P} \text{ in liter per cu m} \qquad (2)$$

The quantity (in volume) of paste in 1 cu m of concrete equals also the volume of cement, additives, and water.

$$V_P = \frac{C}{G_C} + \frac{D}{G_D} + W \quad \text{in liter per cu m} \qquad (3)$$

where C, D, and W are the weights of the cement, additives, and water in 1 cu m of concrete and G_C and G_D the volume weights of the cement and additives.

Indicating the water-cement factor $\omega = W/C$ and the percent of the additives by weight as $d = D/C$ we get:

$$V_P = C\left(\frac{1}{G_C} + \frac{d}{G_D} + \omega\right) \text{in liter per cu m} \qquad (3a)$$

On solving Eq. (2) and (3a) the quantity of cement in concrete will be

$$C = \frac{G_B - G_P}{G_A - G_P} \cdot \frac{1}{\frac{1}{G_C} + \frac{1}{G_D} + \omega} \text{ in kg per cu m} \quad \text{.....(4)}$$

Similarly the quantity of aggregate will be

$$A = \frac{G_B - G_P}{G_A - G_P} \cdot G_A \quad \text{in kg per cu m} \quad \text{.....(5)}$$

Knowing the volume weights of the concrete ingredients, the value (ω), and the percentage of additives (d), we can calculate the quantity of cement and other ingredients. If no additives are used the formulas become simpler. The calculated quantity of cement and aggregates can be regarded as approximate which requires experimental verification as regards workability and density.

Whereas in ordinary concrete the trial of workability is deciding and the volume weight has only an auxiliary significance (for calculating the porosity of fresh concrete), in heavy concrete the trial of density and volume weight are of basic importance not only for examining the properties of radiation shielding but also for choosing the right ingredients.

Usually when dealing with ordinary concrete we regard that the workability depends on the quantity and consistency of the cement paste as well as on the properties of the aggregate. Making a trial with some aggregate mixes which have equal quantity and quality of cement paste, we find that this is the optimal mix which gives the easiest workable concrete. This mix allows decreasing the quantity of the paste and increasing the quantity of aggregate.

As the volume weight of the aggregate in heavy concrete is about 2.5 times higher than the volume weight of the paste, substitution of 10 liters of paste in 1 cu m of concrete, ie., 1 percent of the volume, increases the weight of the concrete by more than 25 kg.

The volume of concrete and its workability can thus be regarded as the main factors in chosing the optimal mix of ingredients by way of experiment.

AUTHOR'S CLOSURE

In their discussion, Messrs. Campbell-Allen and Thorne indicate that there is a thermal disadvantage associated with adding boron to a neutron shield. It should be noted that all of the energy of the rays and particles attenuated are converted to sensible heat regardless of the shielding materials involved, and the temperature rise in the shield is a function of the distribution of heat generation, the system of heat removal, and the thermal properties of the shield.

Adding boron to a shield increases the heat flux in the inner surfaces of the shield due to the capture of thermal neutrons entering the shield. Most of these neutrons would be captured in the first inch of a borated concrete similar to Mix 1 described in the report, compared to about 5 in. of an unborated concrete similar to Mix 3. Boron addition does not significantly affect the distribution of heat generation from fast neutrons and gamma rays. Since in essentially all high flux reactors the inner shield surfaces are metal lined and cooled, it appears advantageous to increase the heat flux in this area by adding boron. This decreases the maximum shield temperature since the effective distance from the heat source to the cooling system is decreased. If there is no artificial shield cooling provided, the reactor is (1) of a low flux type, (2) operated only intermittently, or (3) the design must allow for high temperatures and the resulting thermal expansion problems. If the combined radiation entering a concrete shield is so high that it makes cooling of the shield impracticable, or causes severe radiation damage to the concrete, a "thermal shield" between the radiation source and the concrete shield becomes a necessity.

In each of the reactors described in the report, the borated shielding concrete is placed directly in contact with an aluminum reflector tank which is water cooled when the reactor is operated at its design power for an extended period. The thermal properties of the graphite reflector, the aluminum reflector tank, and the shielding concrete are such that the temperature distribution is relatively flat under both transient and equilibrium conditions.

The information regarding shielding concrete strength requirements is interesting and useful. However, when adding materials which may have serious detrimental affects on the concrete strength, one is primarily concerned about consistently developing enough strength to assure that there will not be excessive surface spalling or other undesirable structural defects.

In the discussion by Dr. Malinowski, he adds useful information regarding setting time, heat of hydration, and methods of preliminary mix calculation. The increased setting time and decreased heat of hydration caused by the addition of typical boron containing materials is considered harmful only when the setting time is excessively increased and the ultimate strength is decreased below a tolerable level. Trial mixes containing boron additives have been experienced which completely failed to set up. Calcium chloride has been added only to correct extreme deviations from the characteristic setting time and moderate heat of hydration of Type II portland cement concretes.

It is quite probable that Type I (normal) or even Type III (high-early-strength) cement could be used along with some boron additives to result in a concrete having moderate heat of hydration properties.

The problem is one of inconsistency in the amounts of water soluble impurities affecting portland concrete setting time, particularly sodium, in the various types and sources of boron containing materials. Some of these materials have little or no effect on the hydration process, and Type II portland cement appears ideal.

A letter has been received from Dyckerhoff Cement Co., Wiesbaden, Germany, indicating that their Dyckerhoff Sulfadur Z-225 cement referred to in Table 1 is a high sulfate resistant cement comparing more closely with Type V portland cement than Type II. It was utilized due to its low heat of hydration and low shrinkage rather than its high sulfate resistant properties.

Discussion of a paper by David Watstein and Robert G. Mathey:

Width of Cracks in Concrete at the Surface of Reinforcing Steel Evaluated by Means of Tensile Bond Specimens*

By BRUCE H. FALCONER†

The maximum bond traction on the embedded length of bar, assuming a cosine distribution of bond traction, is

$$u = \frac{\pi^2}{8} \frac{f_s d}{L} \left(1 - \frac{E_f}{E_c}\right)$$

where

f_s = stress in bar at free ends
d = diameter of bar
L = length of concrete specimen
$\frac{E_f}{E_c}$ = reciprocal of ordinate of Fig. 6

Using the values plotted in Fig. 6 it is computed that, for the increase of bar tension from 20,000 psi to 90,000 psi the bond stress of the ½-in. bar rose from 550 to 1200 psi, and the bond stress of the ⅞-in. bar fell from 950 to 675 psi. The reversed trends are curious.

It is noted that the product of maximum bond stress and diameter tend to constant value for all bars at the 90,000 psi stress. Such bond tractions are consistent with a mean tension of only 150 psi in the concrete.

Fig. 8 shows that at full tension e_c approximates 2/3 e_s. Such extension would suggest a tension of about 3000 psi in the concrete immediately adjoining the bars (assuming a conventional modular ratio). Such anomaly is related to that of Fig. 7.

What happens to the concrete close to the bar within the prism? Does it suffer some kind of low-stress-high-elongation? Is there a narrow annular concertina of cracked concrete around the bar, and contained within the uncracked outer shell?

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Discussion of a paper by Roy W. Carlson and Donald P. Thayer:

Surface Cooling of Mass Concrete to Prevent Cracking*

By ROY R. CLARK, ANTONIO FERRERIRA da SILVEIRA, JAMES A. RHODES, R. S. SANDHU, J. LAGINHA SERAFIM, I. L. TYLER, and AUTHORS

By ROY R. CLARK†

The contents of this paper are of major concern to those engineers who design massive structures of concrete. The concrete technician is usually more interested in producing "good concrete." He may have little concern for design considerations that can and do determine whether a massive structure such as a large dam will be permanently free of structural defects or whether, as in the case of a few large dams in the United States, positive ruptures in areas of greatest stress will shorten the life of the structure.

During 25 years of design and construction of dams, the writer has observed false economies practiced in the design or construction of structures that include: first, a lack of appreciation for basic principles instituted by those in authority; and second, a fear that a few thousand dollars invested in design and construction measures to insure long life will be criticized by someone.

This paper by Messrs. Carlson and Thayer is a serious attempt to contribute to the knowledge and proper design (where it belongs) of massive structures and not leave it to field forces to produce the good concrete that they always consider their basic responsibility. If the large dam at which this study was directed should be built of concrete there is no doubt but that a new and needed experience would result which would contribute greatly to the science of producing massive structures free, or nearly free, of threatening cracks and that greater and maintenance free life would be insured. A few weeks added to the construction period may well add centuries to the useful life of the structure.

The writer knows of no two engineers who can contribute more to the important problem of mass concrete control than the authors of this paper. Both have studied the subject on previous occasions with worthwhile results. Mr. Thayer as a designer and Mr. Carlson as a consultant for Detroit Dam in western Oregon were helpful in de-

*ACI JOURNAL, V. 31, No. 2, Aug. 1959 (*Proceedings* V. 56), p. 107. Disc. 56-9 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).
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termining the program of mass concrete control for that dam which was, and is, so far as the writer knows the most successful program of this kind yet achieved in securing a massive crack free dam that will certainly outlive many other structures of comparable size. The smaller concrete dam should not be ignored in mass control studies. In the Detroit Dam light steel forms were required at the ends of all 5-ft lifts in each of the blocks, about 1200 lifts in all, with the intention of allowing the concrete near the form surfaces to cool rapidly as the internal heat was generated, and thus shrink, or fail to expand, while the concrete was still in a plastic condition. Some designers may differ with this on the basis that initial set takes place in 1 or 2 hr, long before the 3-day period during which the heat of hydration causes temperatures to rise in the mass. There are two answers to this objection: first, the surface concrete does not reach the maximum temperature except in the very warmest weather; and two, in 1200 lifts of the Detroit Dam after careful inspection on each face, vertical and horizontal, of all lifts, no cracks could be seen while exposed. In other words the system worked.

The plan to cool the surface only, if found to be economical, could be a further important step in this problem. The writer fully agrees with the authors that such a condition could prestress the interior mass and cause compressive stresses which would be relaxed at a later date as the interior cools. The catch may be that this cooled zone must be large enough to prevent the interior mass from swelling to crack the skin as its heat builds up. It is probable that before this plan would be adopted for final design and construction, this phase of the problem would be determined for a sufficiently large number of cases to prove its worth.

If satisfactory results can be achieved by this method, or similar procedures, it can be important for futher designs. The writer does not consider the pipe cooling method satisfactory for a large dam, especially if the mass is to be cooled rapidly, say in 2 months, from a high temperature to as low or lower than the final stable temperature. This procedure (pipe cooling) has produced ruptures in large dams in localities of stress and may easily have so changed the monolithic nature that new and unknown stresses have resulted. The writer has, in one case at least, walked through the transverse conduits of a large dam containing vertical cracks parallel with the axis that were so prominent that an "offset" could be seen and felt with the hand which seems to be proof that the structure had already made an adjustment by which the upstream segment had tipped downstream causing listing of the downstream section, a dangerous condition.

One significant statement in the paper is that the maximum temperature of the interior mass concrete would be but 86 F, the mean annual

temperature for this site being 61 F, a difference of 25 F. Also the further statement that if this drop in temperature of the interior mass should be instantaneous it would cause a tensile stress of 625 psi if the restraint was complete. It should be obvious to anyone familiar with mass concrete problems that this interior mass cannot cool rapidly and that creep during a long period of time will greatly reduce such stress and that restraint, as stated, will never be complete. It is obvious that this is the meaning of the authors with which the writer agrees.

From Fig. 2, at a depth of 2 ft there will be a drop of 0 F and at 10 ft depth there will be a drop of 10 deg to final stable temperature. This stable outer shell will tend to prestress the deeper interior as heat is generated and expansive stresses occur. To utilize this principle is, in the writer's opinion, one proper approach to the design of a mass concrete control program for any large dam.

The determination that surface cooling should continue for 19 days is certainly well substantiated by data presented. This would of course introduce some construction difficulties. To raise the forms and allow refrigerated water to flow over the surface for cooling as well as curing should be helpful. Before adopting this plan a temperature calculation of surface concrete would be necessary. To protect the surface concrete from gaining heat from warm outside air, it is thoroughly practicable to require adequate insulation of the surface involved. At Lookout Point Dam in Oregon this was done on two faces of a deep diversion slot in the spillway section, and on one block of the Detroit Dam left exposed for 1 year insulation was required. In both cases when the insulation was removed no cracks or other defects could be seen on close inspection. Other data in the paper are equally interesting and valuable to any engineer concerned with this problem.

In the conclusion to this paper the following statements are made, to which the writer has added his opinion in each case.

a. "The results of this study show that tensile stresses at the surface of massive concrete structures may be reduced by lowering the temperature at which the concrete hardens."—This is true and basically correct.

b. "With this application of surface cooling it should be possible to place monoliths of indefinite lengths without joints or cracks."—This also is true provided proper attention is given to the heat generating properties of the cement and to the cement content of the concrete.

c. "To achieve this result, these studies indicate that the surface cooling must be continued for a period of somewhat more than 2 weeks." —Agreed (see Fig. 3 of the paper).

d. "On the other hand, it is to be noted that this surface cooling may be reduced or possibly eliminated entirely during cooler months of the

year."—This may be true for the Oroville location but in more severe climatic conditions, further study would be required.

e. "The method described herein for computing probable thermal stresses in mass concrete is believed to have a broad application in the field of mass concrete design."—This is undoubtedly true.

f. "The application of this method points to the need for more adequate and more extensive data on the elastic and plastic behavior of concrete."—Yes, this is still true.

g. "The data presented here are believed to be typical for a concrete with a cement content of 3 bags per cu yd."—A low cement content for massive structures has become recognized as proper for massive structures due to studies of this kind by a limited number of engineers.

h. "The control of surface stresses suggests that stresses in other parts of a mass structure might be controlled similarly. Many structures are inefficient because of high stresses in extremely localized regions. The control of concrete during hardening appears to offer a means of controlling the ultimate stresses in such regions, thereby making a more economical design feasible."—The benefits of precooling aggregates, mixing water, and the cement is not to be ignored in any study of a mass concrete control program. At the Detroit Dam where all coarse aggregate was cooled by inundation, the sand in a surface cooled screw type conveyor, and refrigerated water used in the mixer, including some pipe cooling in foundation recesses and other critical zones, the total cost was but 4 percent of the concrete cost or $0.63 per cu yd. The large savings for form costs for vertical joints parallel with the axis, if considered, would reduce the costs of any plan where such vertical joints can be eliminated. An added advantage is a distinct saving in construction time, of increasing importance.

By ANTONIO FERREIRA da SILVEIRA*

The paper presented by Messrs. Carlson and Thayer is a valuable contribution for the problem of controlling the stresses, due to the temperature variations, in a concrete structure. The authors deserve to be congratulated for that reason and their paper should be carefully studied and considered.

The researches carried out by the authors resulted from the need to solve a definite problem. This fact gave a practical quality to the research throughout, and will indeed make it all the more interesting to those who deal with similar problems.

The idea of controlling temperatures in a dam with a view to preventing tensile stresses, which could induce cracks in the structure, is

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not new. But it is a new idea to attempt this control in the faces so as to prevent the appearance of cracks in them especially when the temperature drops to a minimum, or above all when the dam is concreted in hot weather. Thus the method presented is not a method for cooling the concrete mass but a procedure for controlling the appearance of cracks in a given portion of the structure. Consequently it does not replace the cooling of the concrete mass by means, for instance, of cold water flowing in pipes embedded in the concrete, a cooling which is mandatory when it is desired that the dam cools faster than by natural means. Notice that the method leaves the time required for cooling practically unaltered.

On considering such a cooling method, one cannot help fearing that tensile stresses are induced in the external layers as they cool down and that these stresses would be all the more dangerous because meanwhile the core of the dam continues to develop heat. The practical instance presented shows that these stresses are low and therefore there is no danger that the concrete will crack while the treatment is being applied, provided that low cement contents, pozzolanic cement, and precooling are used. Nevertheless if this method is used it should be preceded by a computation of the stresses that will develop. This computation can be carried out by the method described in the same paper which is currently being used by the Bureau of Reclamation. It is also necessary to be quite sure that the mechanical properties of the concrete in the structure are the same as those of the specimens tested in the laboratory from which the elastic and creep characteristics are derived.

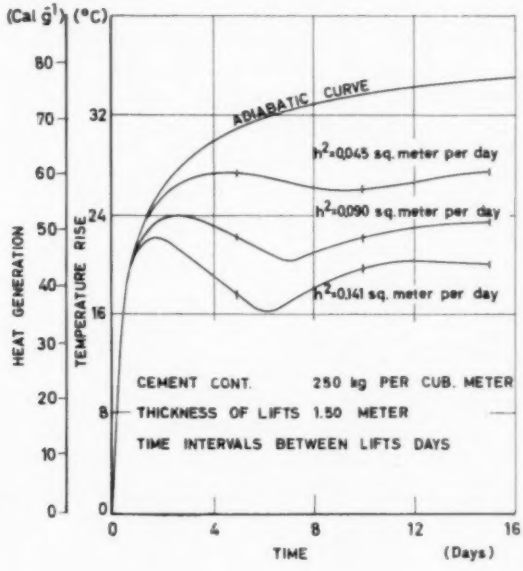

Fig. A — Temperature rise versus time of hydration (in days) for different values of thermal diffusivity (h^2)

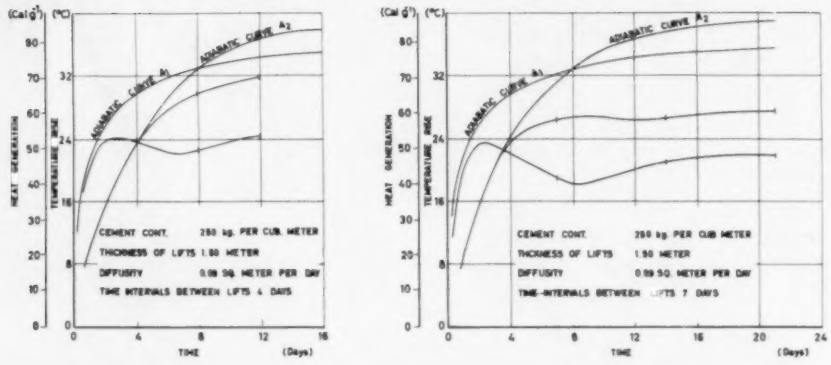
ADIABATIC CURVE A1
ADIABATIC CURVE A2
CEMENT CONT. 250 kg. PER CUB. METER
THICKNESS OF LIFTS 1.50 METER
DIFFUSITY 0.09 SQ. METER PER DAY
TIME INTERVALS BETWEEN LIFTS 4 DAYS
TIME (Days)
CEMENT CONT. 250 kg. PER CUB. METER
THICKNESS OF LIFTS 1.50 METER
DIFFUSITY 0.09 SQ. METER PER DAY
TIME (Days)

Fig. B—Temperature rise versus time of hydration (in days) for concrete with heat generation curves of A_1 and A_2

It suffices that the creep is considerably less marked in the concrete to be applied in the structure than in the specimens, for the stresses to be higher. Additional stresses depend, on the other hand, on temperature and for this the thermal coefficients of the concrete have to be known as accurately as possible.

In fact, the temperature rise obtained is influenced to a considerable extent by the thermal diffusivity of the concrete; this is shown in Fig. A. In the same figure can be seen the temperature rise in the concrete of a dam, concreted with a lift height of 1.50 m, placed at intervals of 5 days, for three different values of the diffusivity (h^2). A cement content of 250 kg per cu m was assumed. The curves presented concern points at the midheight of the lift. The adiabatic curve of the concrete is also drawn in the figure. At another scale, which is indicated, this same curve represents the heat of hydration of the cement. The temperature rise was calculated by Schmidt's method, assuming an unidirectional heat flow, constant temperature values at the boundaries, and a large distance from the foundation.

As can be seen, the temperature rise is considerably higher for low values of h^2.

But the temperature rise also depends to a considerable extent on the curve of heat generation of the cement. Fig. B shows curves of temperature rise, obtained under the conditions and by the methods describe above for concretes with heat generation curves of A_1 *and* A_2, cement contents of 250 kg per cu m, 1.50 m height of lift, and concreted at intervals of 4 and 7 days, respectively. As can be seen, there is a considerable difference between the maximum temperature rises.

According to the authors (p. 111): "Separate studies show that height of lift and rate of construction have only a minor effect on the interior

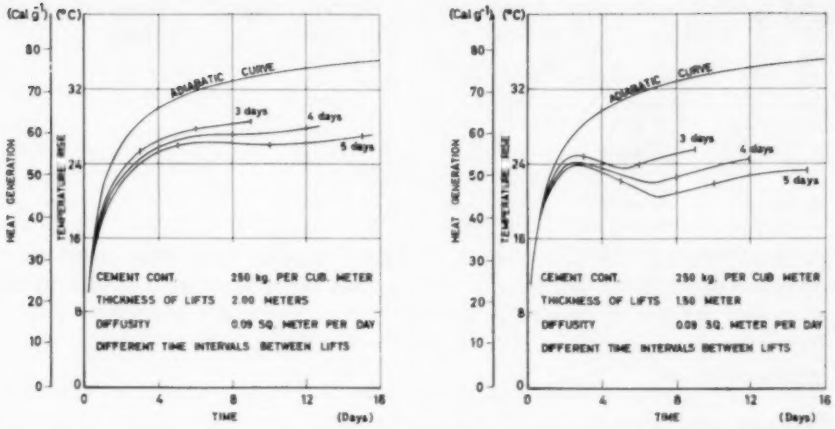

Fig. C—Temperature rise versus time of hydration (in days). Note the effect that height of lift and rate of construction have on temperature rise

temperatures with precooling." This is a statement which is not confirmed by our experience. Fig. C emphasizes the influence not only of the height of lift but also of the rate of construction. The curves, obtained in the conditions and by the method explained above, concern a concrete with a cement content of 250 kg per cu m and indicate the temperature rises observed for heights of lift of 1.50 m and 2.00 m and rates of construction of 3, 4, and 5 days. The variations are also remarkable, as can be seen.

By JAMES A. RHODES*

The authors have successfully translated a theoretical scheme for controlling cracking into practical application. The surface cooling procedure proposed does not conflict with the surface insulation methods currently used by the Corps of Engineers, but is rather an alternate approach to the problem.

Surface heat transfer coefficient

Calculation of temperature gradients near the surface by the Binder method indicates that a 40 F concrete surface temperature can be attained with cooling water at about 38 F. Available heat transfer data for temperatures of this magnitude give a coefficient of about 15 Btu per sq ft per hr per F. During the first 6 days in the authors' example, surface coefficients averaged from 28 to 37 Btu per sq ft per hr, only slightly greater than the 30 Btu rate this writer estimates would exist. The difference would have little significant effect on the authors' re-

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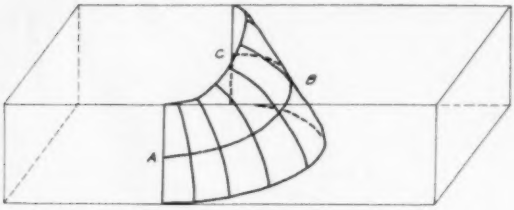


Fig. D — Temperature surface

sults. Prototype measurements of heat transfer from the surfaces of completed concrete dams have shown that rates of from 1 to 5 Btu per sq ft per hr per F generally prevail for concrete-air transfer. The 15 Btu rate for concrete-water transfer coefficient thus appears to be reasonable.

Restraint conditions

Both Condition A, internal restraint only, and Condition B, foundation restraint only, are based on the premises of no deflection and plane sections remaining plane during temperature changes. Each premise is a convenient and logical assumption for the purposes of the study. Condition B, however, does not recognize the simultaneous existence of internal restraint (similar to Condition A), which would be additive in its effect. The actual tensile stresses under Condition B for the first 19 days would likely be greater than indicated, and could approach the Condition A stresses. Cracking experienced (without special surface cooling) in mass concrete dams occurs as frequently in low lifts adjacent to the foundation as in the more remote lifts. This would tend to support the supposition that there is little difference in tensile stresses at the two locations.

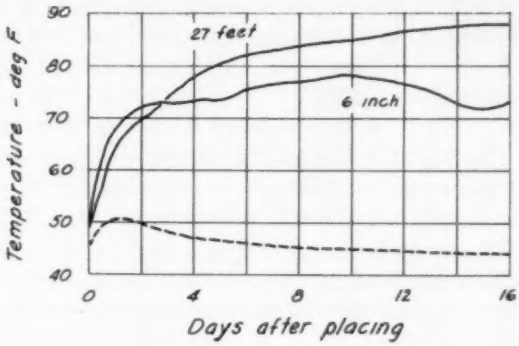
Average lift temperatures

Strains for Condition A restraint are based in part on what is called "the average temperature of the whole lift of concrete." A rigid interpretation of this phrase would mean a single temperature value which would represent the entire temperature surface shown in Fig. D. Perhaps a less complex interpretation would be the average temperature transversely across the lift at plane ABC. It would be helpful if the authors would explain how the average lift temperatures were obtained, and if equilibrium of forces, deformation, and deflection requirements were considered in determining the resulting strains.

Creep coefficients

The creep data used for converting thermal strains to stress is interesting in that the creep curves at the various loading ages do not bear the same relationship usually reported for creep studies. It is understood that the adjustments made to the laboratory data were

Fig. E—Summer conditions



necessary to obtain reasonable and consistent stress histories, and that early age creep assumed considerable importance because of the temperature manipulations during the 19-day cooling period. If the creep coefficients given in Table 2 can be proven valid, the utilization of creep in concrete design will be simplified in several ways. First, a complete family of creep curves for a given concrete can be derived from one specimen loaded at an early age, with instantaneous values of E obtained by sudden unloading and reloading at intervals. Second, comparison of creep characteristics of different concretes can be easily accomplished by two curves, the creep strain history for load applied at 1-day age and the 1/ (instantaneous E) history. Third, the net stress existing at any age can be determined simply from the rate of change of creep strain at that age, providing the creep strain rate is measured over a period during which it can be assumed the total stress is constant. Detailed discussion of the creep coefficients is beyond the scope of the paper, but it would be interesting to see the approximate magnitude of the adjustments that were made to the laboratory data.

Uncooled surfaces in prototype structures

The reduction in concrete temperatures that could be attained by surface cooling during hot weather is shown graphically in Fig. E. The

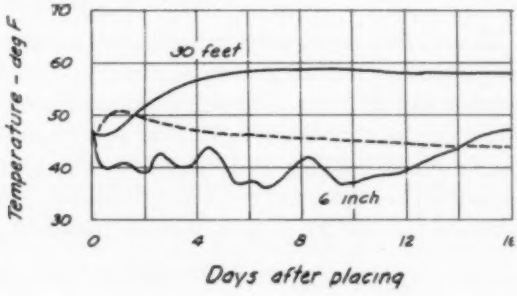


Fig. F—Winter conditions

actual prototype temperatures are given by the upper two solid curves, measured at Table Rock Dam (in southern Missouri) for a 7.5-ft lift of 2.5-bag mass concrete placed on August 13 with an initial concrete temperature of 49 F. The single lower dashed curve is the 6-in. depth history transposed directly from Fig. 2 of the authors' paper.

On Fig. F measured temperatures near the surface for a November 12 placement at Table Rock Dam are shown, with the unlabelled dashed line curve again being the authors' 6-in. depth history.

These figures demonstrate that while a considerable cooling effect can be attained for hot weather placement, the proposed surface cooling scheme need only be utilized for lifts placed during the summer season.

Corps insulation practices

On mass concrete construction the Corps of Engineers currently require that bulkhead surfaces exposed more than about 15 days during the winter season be insulated for the balance of the winter. For concrete placed during the summer a moderate amount of tension at these surfaces is recognized as inevitable and is tolerated during the hot weather, but subsequent excessive decreases in surface temperatures (both sudden and seasonal) which would result in additional tensile stresses are minimized. For winter-placed concrete, the 15-day uninsulated period provides an opportunity for the natural development of some tension at early ages, with a tendency for a change to compression following installation of insulation. The similarity of this stress development sequence to the surface cooling scheme is obvious, and in addition the adverse effects of excessive surface temperature drops are avoided.

After concrete is placed in the adjacent monolith, the temperature of the previously insulated surface increases, incremental compression stress is developed, and the entire concrete mass then undergoes temperature adjustments at a rate which allows creep to modify the intensity of the thermal stresses in the interior of the structure.

By R. S. SANDHU*

The authors are to be congratulated for an illuminating paper on an important aspect of concrete control. The idea of surface cooling intended to bring the exposed surface of concrete mass to approximately the mean annual air temperature has been completely described. Without surface cooling and under the conditions expected to occur at Oroville Dam site, the final temperature drop would be from a no stress temperature of 83 F to a winter temperature of 40 F, whereas with

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surface cooling the drop would be from 60 F to 40 F, i.e., a drop of only 20 deg. With surface cooling it is stated that placement of concrete masses of even 800 ft length would be possible without verticle construction joints and without any serious risk of cracking in the mass. If so, this would eliminate the necessity of providing artificial cooling of the interior mass of concrete and in most cases of the complicated grouting arrangements for vertical joints.

There are a few apparent difficulties on which the authors may wish to throw further light.

(1) According to the authors, because of the slow rate of cooling of the interior concrete, tensile stresses will not be very high. Actually, in most structures, the restraint offered to volume change is due to the foundation rock. This restraint will restrict shrinkage or expansion parallel to the restraining surface but will have practically no effect in the direction normal to it. For instance, on a horizontal foundation there will be no restraint to volume change in the vertical direction and very little in the horizontal direction except for the lower few lifts. If under these conditions there is a temperature drop in the interior concrete and if the mass changing temperature is large, instead of the interior concrete developing tensions, it may actually remain in a state of no or small tensile stresses (forces other than those due to temperature changes being ignored) and cause heavy compressive forces in the concrete near the cold surfaces which either have to compensate for the volume change or spall. Considering the equilibrium of forces at a horizontal section would show that any development of tensile stresses over a large area in the interior would be compensated for by much larger compressive stresses in the concrete near the exposed surfaces. It would be interesting to know if this aspect was also analyzed and if so with what results.

(2) Under restraint Condition A, tensile stresses rise rapidly to 150 psi at 8 days. Would 3-bag concrete containing 30 percent pozzolan replacement be able to stand these stresses at the corresponding age or is any special treatment considered desirable?

(3) In the opinion of the writer, prestressing of surface concrete does not solve the problem completely. In fact, near the free boundaries such prestress may not exist at all in spite of surface cooling. Cooling of one part while the major portion of the mass is still hot may result in an undesirable redistribution of stresses. Surface cooling would appear to be much more effective if used with some other method which should restrict the rise of temperature in the interior concrete as well. If the embedded cooling systems and elaborate joint grouting programs are to be avoided, the treatment proposed has to restrict heat generation and remove the heat generated. The authors have recommended use of Type II cement, Pozzolan, precooling, and surface cooling. If in addition and during the hotter part of the year, the height of lift were to be reduced,

and after hardening of concrete surface, it was flooded with water for a few days, a large amount of heat generated would be extracted. For this purpose, assuming 16 days placement of 10-ft lifts, i.e., 8 days for say half the height, flooding with cold water would not require any special forms, equipment, or apparatus and would restrict the maximum temperature to one substantially lower than the 86 deg calculated by the authors for 10-ft lifts. The final drop in temperature from this reduced temperature to 61 F would be slower than if the temperature were 86 F and this would restrict the rise in compressive stresses mentioned in Paragraph 1 and they therefore may be within safe limits for the material used. Using this expedient along with surface cooling may ensure more positive realization of all the advantages visualized in the paper without introducing undesirable stress redistributions.

By J. LAGINHA SERAFIM*

The knowledge gained in Portugal by observing concrete dams has shown that the natural cooling of the faces accounts for important compressive stresses which explain the small filtration of water observed. Stresses observed 1 m from the faces in Cabril Dam were reported elsewhere.[1]

Although definite conclusions can not yet be drawn, it seems that the diffusion of water from the reservoir inside the pores of the concrete, as observed by Carlson-Terzaghi pore pressure meters, is much faster in a dam cooled by embedded pipes than in a dam without artificial cooling. Thus, it can be advanced that if the faces of the concrete are kept at a low temperature when the temperature of the interior of the block is lowering, initial compressive stresses in the faces of the concrete can develop which not only will prevent cracking and deterioration but also will decrease the permeability. The interest of external cooling presented in the paper is thus manifest, provided, as the authors state, that no initial cracking takes place during the cooling of the faces. The writer is especially interested in such a technique for the construction of thin arch dams.

The total time of cooling, the temperature of the faces of concrete, and the age at which the cooling must start, are the most important unknowns that must be determined, taking into consideration the rheological properties of concrete and their variation with age, time, temperature, and humidity.

In the computations of the stresses due to the temperature strains the method of integration by steps, as developed at the Bureau of Reclamation, was used by the author. It seems interesting to call attention here to other mathematical methods used in eastern Europe[2,3] for solving

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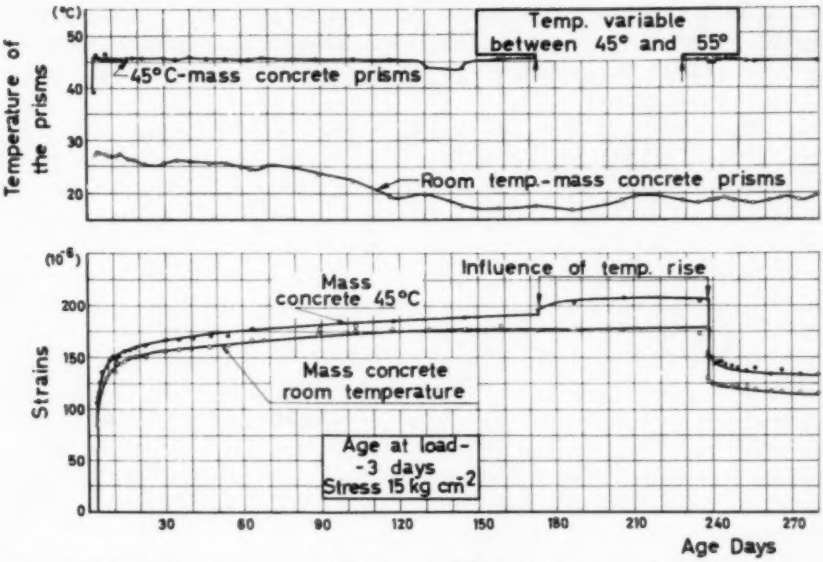

Fig. G—Creep of mass concrete prisms loaded at the age of 3 days, at room temperature and 45 C, first series

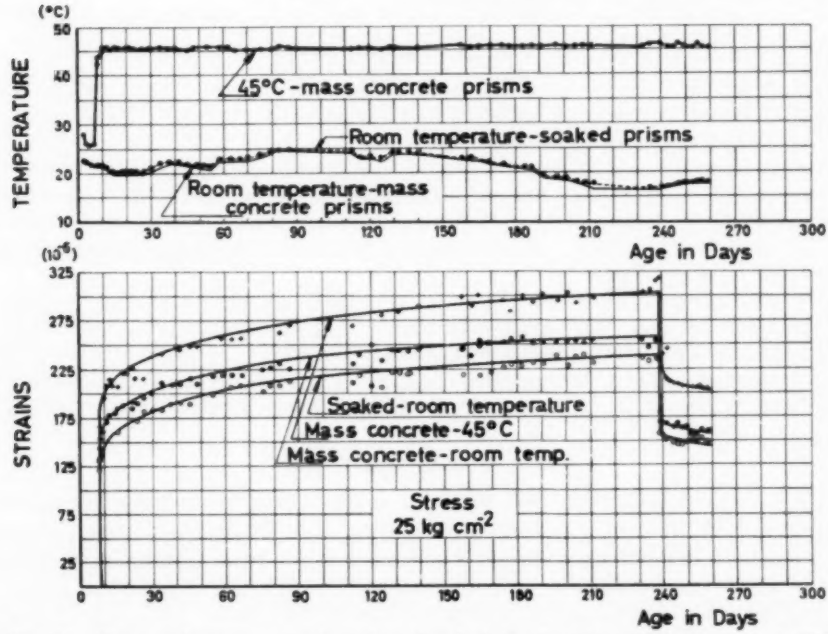

Fig. H—Creep of mass concrete prisms loaded at room temperature and at 45 C, and of water soaked concrete at room temperature, second series

similar problems. However, in all those computations it is assumed that the creep characteristics of the concrete are the same across the thickness regardless of the temperature and the degree of dryness of the concrete at the various points.

Tests underway at the Laboratorio Nacional de Engenharia Civil, in Lisbon, show that there is a noticeable influence of the temperature on the creep of mass concrete, especially during the first days of loading when the rate of creep is larger for the higher temperatures.[4] Results also show that any temperature rise increases the rate of creep (Fig. G). In another series of tests it has also been observed that prisms cured in water (soaked 3 days after casting) show a lower creep than mass cured prisms (Fig. H).

The writer looked at the diagram of previous determinations of temperature stresses made at the Bureau of Reclamation[5] for prisms kept at constant length using the same method as that followed by the author. Although time was not enough for making a new calculation of the stresses considering the variation of creep with temperature it seems that such an assumption would probably bring closer the computed stresses in those prisms with the observed ones.

In the case of surface cooling it is thus questioned if lower temperatures of the faces will not bring higher tensile stresses, during the period of surface cooling, than those computed.

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2. Babuska, I., and Mejzlik, L., "Calculation and Measurement of Thermal Stresses in Gravity Dams," *Report* No. 59, Sixth Congress on Large Dams, New York, 1958.

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By I. L. TYLER*

The method for prevention of temperature cracking in mass concrete, proposed by Messrs. Carlson and Thayer, is a brilliant approach to a solution of the old problem of cracking due to heat liberation by the cement during hardening. Whether or not the scheme is practical under construction conditions has yet to be proved but there is enough back-

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ground information to suggest strongly that it is feasible, particularly under Condition A as described by the authors.

The rate of change in shape of the temperature curve between the exposed surface and the higher temperature interior concrete, and the age of the concrete at the time the change takes place are known to influence cracking tendencies greatly. The writer is familiar with one case in which embedded cooling pipes were used successfully reduce the gradient of the temperature curve from the interior toward one exposed contraction joint face of a long construction block in a gravity dam.

In this instance 1-in. cooling pipes were placed at the tops of 5-ft lifts. Horizontal spacings between pipes were such as to permit maintaining a nearly straight line gradient in temperature for about 20 ft from the long contraction joint toward the center of the monolith. The first pipe was about 1 ft from the contraction joint and the pipe spacings increased away from the joint. The opposite side of the monolith was cast against an already constructed portion of the dam and no artificial cooling was considered necessary. The artificial cooling was maintained adjacent to the one contraction joint between elevations of about 50 and 130 ft above foundation. That the cooling procedure was effective seemed evident from the nearly crack-free surface produced, compared with the prevalent vertical cracking both above and below the cooled section.

Although the cooling layout was in no way comparable with that proposed by the authors of this paper, it would appear that some of the benefits of the Carlson-Thayer method were operating toward controlling cracking. Since cooling water, somewhat below concrete temperature, was started early in the life of the concrete, some of the creep effects at early ages may have been present though the 5-ft vertical spacing of cooling pipes would have reduced its effect considerably. Cement was of low heat composition, 0.8 bbl per cu yd.

It is the writer's opinion that this experience along with many other attempts to reduce temperature cracking by controlling temperature gradients, rather than maximum temperature rise only, tend to strengthen the validity of the proposals so ably set forth for the first time by Messrs. Carlson and Thayer.

AUTHORS' CLOSURE

The authors are deeply gratified at the widespread response evoked by this paper and wish to thank all those who have contributed to the discussion.

The description presented by Mr. Tyler of an instance of localized cooling and the results obtained thereby are appreciated. Although, as he states, this is not the same as the surface cooling proposed by the

authors, it does, in a measure, point to probable success of the surface cooling method.

The apprehension felt by Mr. Sandhu of high compressive stresses near the surface of the concrete is not shared by the authors. It will be noted from the lower diagram of Fig. 3 that this stress amounts to somewhat less than 500 psi at an age of 40 days with the trend of the curves indicating a still lower value at later dates. This stress is certainly not a large proportion of the ultimate strength of even the leanest concretes used and would appear to be well within the margin allowed for in the "factor of safety" usually adopted for design. Failures in compression due to temperature change are practically nonexistent in mass concrete structures as far as the authors have observed.

The computed tensile stress of 150 psi at 8 days' age is not believed likely to cause rupture. More information is needed, however, on the strengths of concrete at early ages to prove this point. The authors agree with Mr. Sandhu that surface cooling itself does not solve the problem completely. At this point, it must be emphasized that the study presented in this paper was made for construction with specific materials at a specific site subject to the temperature cycle shown in Fig. 1. Under these conditions, the temperature of the concrete closely approaches the ambient temperature after a period of about 3 days. For this reason, cooling the horizontal exposed surface during the time it would be exposed would not contribute greatly to removing heat from the mass. Under a different temperature cycle, possibly one with which Mr. Sandhu is more intimately acquainted, such cooling might prove to be very beneficial.

Mr. Clark's endorsement of the surface cooling proposal is appreciated. His description of the experience at Detroit Dam with metal forms where, in effect, surface cooling was provided by nature is an interesting confirmation. His experience of the use of insulation on exposed surfaces at Lookout Point Dam is also especially interesting. It was anticipated that if Oroville Dam were to be built of concrete, insulation of concrete surfaces would have been specified in certain appropriate places.

The approximate confirmation of the heat transfer rate assumed by the authors by the data presented by Mr. Rhodes is interesting. Mr. Rhodes inquires further as to the exact interpretation of the "average temperature of mass" used by the authors. The actual temperature used was the average transversely across the lift at midheight. The creep coefficients used in the studies were substantially as measured in the laboratories of the University of California. Such adjustments as were made were certainly not radical but only of a magnitude to smooth out observational and experimental errors. The significance of this adjustment can best

be visualized by inspection of Fig. 3 of Mr. Raphael's paper.* As will be seen from this three dimensional view of the "creep surface" adjustment of the vertical ordinates to a smooth surface which is consistent with all of the observed ordinates would not constitute a radical adjustment and will be consistent with the observations as a whole.

The authors appreciate the comparison with actual temperatures at Table Rock Dam as presented by Mr. Rhodes. This serves to point out two facts: (1) the substantial benefit obtained by surface cooling during the warmer months of the year and (2) the lack of need of surface cooling during the cooler portion of the year. Mr. Rhodes mentions, as did Mr. Clark, the use of insulation on exposed surfaces and the same comments made heretofore apply.

Mr. Serafim mentions the possible effect of the surface cooling on permeability of the concrete. This is a point which has not heretofore been considered but which should be worthy of further laboratory investigation.

The effect of temperature on the creep of concrete as presented by Mr. Serafim is a valuable contribution. A significant temperature effect on creep would certainly modify the results of computations such as those presented by the authors and should be taken into consideration. This matter is to be explored by further studies at the University of California.

Mr. da Silveira raises several important points which should be considered. It is perfectly true as he states that the surface cooling method proposed by the authors leaves the time required for the cooling of the entire mass practically unaltered. In the case of structures where final volume change after cooling to the mean annual ambient temperature is a consideration, such as a tunnel plug, the method of surface cooling would be quite inapplicable. This would also seem to be true in the case of an arch dam except that the authors believe that, by the selective use of surface cooling, a favorable distribution of stresses after final cooling might be achieved.

Mr. da Silveira's observation that the properties of the concrete actually being used in construction must be accurately known is, of course, entirely correct; such knowledge, in fact, is a vitally essential element in the planning of any concrete program. The data which he has so kindly presented points out the effect of diffusivity, height of lift, and interval between placements on the concrete temperatures.

The statement by the authors that the height of lift and rate of construction does not have a significant effect on the temperature was meant to apply to the particular conditions being studied and not as a general observation. Under conditions where the adiabatic temperature rises considerably above the ambient being considered, as would be the

*loc. cit.

case without precooling, the rate of construction obviously would have a significant effect.

Since the surface cooling of concrete proposed by the authors is a departure from the usual techniques of design, insufficient information is available on the particular properties of mass concrete which apply. This is particularly true of the temperature effect on the rate of creep, the tensile strength of concrete at early ages, and the creep rate of concrete under tensile stress. Data on these and other important properties of concrete will have to be the subject of further laboratory studies.

Discussion of a paper by Ignacio Martin and Sixto Ruiz:

Folded Plate Raft Foundation for 24-Story Building*

By JACOB FELD†

The writer is glad to see this valuable information made available to the technical reader. The clear, concise, yet complete, presentation of the problem, the decision of design form and of the execution is another example of the advances made in structural engineering by the firm in which the authors are associated. Historically, the folded plate raft is a worthy successor to the crossed timber log raft used in soft wet soils over a century ago, followed by the brick arch mats in the latter part of the 19th century.

The choice of 5000 psf bearing value under full load, including wind, for the mat supporting the tower as against 6000 psf for the individual footings of the wings is an important point which should be kept in mind when one uses such a combination of foundation design. For equal settlements under load, the smaller size footings will sustain greater unit loading. Differential settlements are too seldom considered in American design. The USSR Building Code lists a series of permissible maximum differential settlements within several types of structures. Other foreign standards likewise suggest similar limits. The authors' decision to design the junction between tower and wings to resist a differential settlement of 1 in. is a wise expenditure of a small amount of additional cost. Ugly cracks in such shaped buildings often result from the disregard of this well known action of various sized and nonuniformly loaded footings.

The choice of a folded plate was certainly a wise one, even though no precedence existed. With the six rigid shear walls transmitting the major part of the tower load as continuous reactions for the soil resistance, the design is logical, direct, and therefore good engineering. Since the folded plate is a much more economical concrete span than a flat slab, its choice is justified, as is definitely proven by the comparative estimates.

The authors must be congratulated on this novel design which should become a standard solution for similar loadings and soil conditions. Additional compliments are in order for the form and brevity of presentation.

*ACI JOURNAL, V. 31, No. 2, Aug. 1959 (*Proceedings* V. 56), p. 121. Disc. 56-10 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).
†Member American Concrete Institute, Consulting Engineer, New York, N. Y.

Discussion of a paper by John F. Brotchie:

General Elastic Analysis of Flat Slabs and Plates*

By DANIEL FREDERICK and AUTHOR

By DANIEL FREDERICK†

This discusser has read with interest the current paper by Mr. Brotchie and his previous one with discussion[1] in which he presents a new and simplified method for analyzing flat slabs and plates. In his most recent paper, he mentions that the method is applicable to any linear theory where secondary effects such as shear deformation might be included.

It is the purpose of this discussion to bring to the attention of those readers of the ACI JOURNAL who may not be aware of it, the Reissner[9] plate theory which provides important modifications to the classical thin-plate theory. It is one of the refined theories to which Mr. Brotchie's method would be applicable. The important features of the Reissner theory are: (a) it includes the effect of shear deformation and normal pressure and therefore might give better results in regions where shearing forces are large such as the region near a column; and (b) it permits the satisfaction of three boundary conditions on each edge of the plate instead of two as in the classical theory and would be expected to give better values for stress concentration factors among other improvements. Several important rectangular and circular plate problems have been solved using this refined theory.[17,18]

Using polar coordinates, the governing equation for the deflection of a circular plate resting on an elastic foundation of modulus k, using Reissner's theory is

$$D\,\nabla^4 w - \beta h^2\,\nabla^2 (q + kw) + (q + kw) = 0 \qquad (23)$$

where w = transverse plate deflection

h = plate thickness

q = lateral applied pressure

$$\beta = \frac{2 - \mu}{10(1-\mu)}$$

μ = Poisson's ratio

$$\nabla^2 = \frac{\partial^2}{\partial r^2} + \frac{1}{r}\,\frac{\partial}{\partial r} + \frac{1}{r^2}\,\frac{\partial^2}{\partial \theta^2}$$

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There is also a second order differential equation governing a stress function ϕ.[17,18]

The homogeneous solution of Eq. (23) can be written in terms of Bessel functions of the first and second kind with a complex argument

$$J_n\ (xe^{i\,\alpha/2}) \qquad J_n\ (xe^{-i\,\alpha/2})$$
$$Y_n\ (xe^{i\,\alpha/2}) \qquad Y_n\ (xe^{-i\,\alpha/2})$$

where $x = r/l$

$$l = \sqrt[4]{\frac{D}{k}}$$

$$\alpha = \cos^{-1}\left(-\frac{h^2\beta}{l^2}\right)$$

Mathematically, these are similar to the functions in Mr. Brotchie's paper and are handled in the same way. Expressions for the moments and shear forces in terms of these functions and other details involved in solution of boundary value problems can be found in References 17 and 18.

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AUTHOR'S CLOSURE

The author is pleased to receive Professor Frederick's discussion and considers it to be a valuable addition to the paper.

Discussion of a paper by A. M. Neville:

Creep Recovery of Mortars Made with Different Cements*

By A. de SOUSA COUTINHO, R. D. DAVIES, and AUTHOR

By A. de SOUSA COUTINHO†

The essential experimental facts mentioned by the author were also observed by us a few years ago, i.e., small magnitude of creep recovery, its swiftness and irreversibility and, above all, a fact of the utmost importance which was also observed by some other workers: the fact that the loss of water is exactly the same in a specimen under creep as in the unloaded companion specimen. In our tests we could even determine the distribution of humidity along the specimen under creep test. This was determined by a method in which the variation of electrical resistance between metallic electrodes, placed along the cross section during the casting of the specimen, was measured. These tests showed that the variation of electrical resistance along the loaded specimen is exactly equal to the variation of electrical resistance along the unloaded companion specimen; the humidity distribution was exactly equal in both specimens. Therefore, the evaporation and the movement of water inside the specimen was not at all disturbed by creep phenomena.

However as regards the interpretation of creep, we do not entirely agree with the author.

Although the exact nature of the mechanism of creep remains unknown, as the author points out in the discussion of results, the author suggests that it may be due to the movement of the zeolitic water from the calcium silicate hydrate into the capillary water.

The author says that the chemical composition of cement is not a primary factor in creep, stating that "there is no significant correlation between chemical composition of cement and creep of mortar, although some correlation between chemical composition and strength has been obtained." Now, if a correlation exists between chemical composition and strength, and between strength and creep, then a correlation must exist between the chemical composition and creep. Unsuccessful attempts

*ACI Journal, V. 31, No. 2, Aug. 1959 (*Proceedings* V. 56), p. 167. Disc. 56-13 is a part of copyrighted Journal of the American Concrete Institute, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).

†Research Engineer, Head, Concrete and Cementitious Materials Division, Laboratorio Nacional de Engenharia Civil, Lisbon, Portugal.

to prove this correlation are due no doubt to the rough method used; a linear correlation between cement composition and strength or any other property is a rough and even doubtful approximation of reality, as cement is not a sum of elementary constituents but a true combination of these.

We do not think that the mechanism of creep is related to the movement of zeolitic water from the calcium silicate hydrate or from the cement paste into the capillary pores in the gel. This can not explain the correlation between strength and creep.

We believe, as Le Chatelier suggests in his study of the mechanism of decomposition of cement due to the expansion of calcium sulfoaluminate,[15] that *the stress acting on the hydrated silicate or aluminate in contact with a liquid in which it is soluble influences its coefficient of solubility.* Thus when the cement paste is subjected to a stress, its constituents are partly dissolved or hydrated under this stress, which modifies the hydration conditions of cement, and this hydration, extremely slow, induces a progressive strain in the mass. This has the advantage of explaining all the experimental properties of creep that are so far known, except, unhappily, one. In the main these properties are:

(a) Low values of creep correspond to high strengths. This is because in high-strength cement or concrete the hydration is much faster because a higher percentage of hydrated cement exists, whereas in low-strength concrete[16] there is much more paste to be hydrated and this means that the movements will be greater.

(b) A strength increase is observed in a specimen subjected to creep after the test. Although small, this increase occurs and has been observed by many authors.[17]

(c) There is an absence of transversal movements during creep. When stress is applied in a given direction, the variation of solubility of the cement components or of the hydration of the cement paste takes place along the direction of the applied stress, and that is why movements do not occur in the other directions.

(d) There is an irreversibility of creep, i.e., when stress vanishes the strain does not, and a certain residual strain remains. This can be explained by the irreversibility of cement hydration.

(e) The exact correspondence between relaxation and creep, the properties of both phenomena (the same laws of variation with time and with the strength) make us think that both phenomena have the same cause, i.e., hydration or variation of the solubility with the stress in the cement constituents.

Although the possibility of relating creep to the hydration of cement paste under stress is extremely attractive, a fact that unhappily cannot be explained is the influence of the transverse dimensions of the specimen on the value of creep.

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By R. D. DAVIES*

I have read this paper with interest. Among the important results are the values of elastic strain and instantaneous recovery given in the fourth and fifth columns of Tables 1 and 2. How accurate does the author consider these values to be, and how did he obtain them? His description of his apparatus in a previous paper in the *Magazine of Concrete Research*[9] suggests that several minutes must have elapsed between the beginning of loading (or unloading) and the completion of the first strain measurement. It is well known that initially creep is comparatively rapid, and Evans[18] has shown that a good deal of creep occurs during loading unless this is completed in about a hundredth of a second. Did the author use some method of extrapolation?

REFERENCE

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AUTHOR'S CLOSURE

Dr. Davies is correct in his interpretation of the method of loading and unloading the specimens tested by the author. The creep during loading could therefore not be measured, and extrapolation to estimate it, because of the assumptions involved, was not considered to lead to results of greater value.

It may be added that the values of the modulus of elasticity were obtained by loading at the same rate as that of the creep specimens but

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three load cycles were applied prior to measurement. For different values of sustained stress the time taken to apply and release the load was proportional to the stress applied.

Once again it is a pleasure to read Mr. Coutinho's contribution, and to hear that his experimental results are in close agreement with those of the author. As far as the nature of creep is concerned, no conclusive experimental evidence seems to be available as yet and, in the author's opinion, the suggestion of the solubility effects is not substantiated.

Discussion of a paper by William Hansell and George Winter:

Lateral Stability of Reinforced Concrete Beams*

By C. S. BUMANN, W. T. MARSHALL, R. B. L. SMITH, and AUTHORS

By C. S. BUMANN†

The authors have presented an interesting and timely paper and have given results of tests concerning a subject about which accurate knowledge is sadly lacking.

It is very probable that the requirements of Section 704 of the ACI Code are too restrictive for rectangular beams with low d/b ratios and which have continuous or closely spaced lateral support for the tension areas. The most common beams of this type are turned-up beams with a roof or floor slab as a bottom flange. However, where the d/b ratio is great or where there is a highly stressed compression flange with a relatively thin web the ACI Code may not be too restrictive.. It probably is conservative, but in regard to a matter of which we know so little it is best to be conservative. If the authors' paper creates enough interest that further tests and investigations are made it will have accomplished a great deal.

However, it is the writer's opinion that the test results given in this paper are not valid for use as a guide to safe design. This is because of the yoke arrangement used to join the tension and compression areas of the beams at the loading points. The tops of the beams were in compression, which would tend to cause lateral deflection, while the bottoms were in tension, which would tend to prevent such deflection. The yokes, by joining the top to the bottom, would reduce the lateral deflection of the top and increase it at the bottom. The yokes would thus supply a large amount of lateral support to the top or critical portion of the beam by connecting it to the bottom which is held rigid by tension in the reinforcement. It seems likely that failure would have occurred much sooner if the yokes had not been used.

It may be that without the yokes failure would have occurred at too early a stage to be a fair test of the capacity of the beam. Any slight initial deflection would be amplified by the application of the load to the top instead of to the bottom and early failure induced. In practice such beams are usually loaded at the bottom.

*ACI JOURNAL, V. 31, No. 3, Sept. 1959 (*Proceedings* V. 56), p. 193. Disc. 56-14 is part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).
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It is the writer's opinion that future tests should be made by applying the loads to the bottom of the beam, perhaps by means of eye bolts or some similar device. This would leave the top entirely without lateral support and at the same time without any load tending to increase such lateral deflection as might occur from compression. Such application of load would tend to reduce lateral deflection at the bottom but this need cause no concern since in practice in most cases where the top of the beam is without lateral support the load is applied to the bottom and the bottom also has rigid lateral support.

By W. T. MARSHALL*

The problem of calculating the buckling load of a slender beam made from reinforced concrete is as the authors rightly say not an easy one, because of the complex nature of the material. It involves in the main a knowledge of: (1) the flexural rigidity EI_y of such a beam, and (2) the torsional rigidity GK_t.

The writer has carried out experiments (16, 17) to determine these and has established reasonable values for them at working loads under laboratory conditions. As the load is gradually increased the values will fall off slightly but near failure there will be an appreciable drop. The problem of calculating the buckling load however does not necessarily involve the values of the above mentioned functions near the failing load in flexure since buckling is not a gradual failure like flexure, but more of an instantaneous type.

In common with the authors, the writer did not agree with the codes which base instability solely on the L/b ratio and in particular the clause in the British Standard Code of Practice which allows no load to be taken on a beam where the L/b ratio exceeds 70. The authors correctly point out that the values given by the writer in his paper[2] are on the conservative side, but even so they show two weaknesses in the code, namely that the lateral instability load involves the dimension d as well as b and and that even with high values of d/b the L/b given by the code are too conservative.

The writer is therefore grateful to the authors for once again drawing attention to the weakness in the codes and expresses the hope that those responsible in America if not in Britain will recognize this fact.

There are three points in the paper on which the writer would like to comment.

1. The analysis given is taken from Timoshenko and gives the critical moment for a beam subjected to a constant bending moment throughout *its entire length*. The beams used in the experiments however were not

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so loaded. The bending moment was only uniform over the central half of the beam, the portions for a distance $L/4$ from each support being subjected to a varying bending moment. Fortunately this error makes little difference to the result because the collapse load for an instability failure in the case of a beam loaded at the quarter points is $24.1/l^2 \times \sqrt{EI_y GK_T}$ corresponding to a maximum moment of approximately $3/l^2 \times \sqrt{EI_y GK_T}$.

2. The analysis given by Timoshenko applies to slender beams only. The analysis given by Michell[18] gives the load to produce buckling as

$$\frac{m}{L^2}\sqrt{\frac{EI_xI_yGK_T}{(I_x - I_y)}}$$

(assuming that E is the same in both x and y directions), m being a coefficient depending on the type of loading. In the case of slender beams $I_x - I_y = I_x$ and the simplified expression given in the paper is obtained. One could not possibly use this simplified expression as the authors suggest on p. 203.

3. The authors seem surprised that failure by buckling did not take place. A simplified calculation of the buckling load would have shown them that instability failure of beams of the dimensions used would not take place even on the longest span before flexural failure occurred.

This simplified calculation was carried out by the writer as follows:

Take $E=2.5 \times 10^3$ kips per sq in. and $G = 1.2 \times 10^3$ kips per sq in. (Both of these values are low for laboratory work.)

Take $I_y = db^3/12$ and $K_T = 0.3b^3d$. (Cracking will naturally reduce these values but the reduction here may be compensated for by the low values taken for E and G and also the neglect of the reinforcement in assessing the values of I_y and K_T.)

The value of the load applied at quarter points to produce failure can then be shown to be approximately $9320/l^2$ kips where l is in feet. The values for spans of 12, 15, and 18 ft are 64.8, 41.5, and 28.8 kips, respectively. These are so much greater than the probable failure load in flexure that it is extremely unlikely that instability failures will occur. If the authors wish to produce failures of this type then narrower beams should be used in the experiments.

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18. Michell, A. G. M., "The Elastic Stability of Long Beams under Transverse Forces," *The Philosophical Magazine* (London), V. 48, No. 292.

R. B. L. SMITH*

As mentioned by the authors in their introduction, lateral instability is a less serious problem in design of reinforced concrete than in structural steel (or also in alloys or prestressed concrete). Most reinforced concrete beams may be completely restrained from buckling, as T-beams, or rectangular beams with bonded floor construction superimposed. Yet regulations for slender reinforced concrete beams exist in most countries, usually based on the L/b ratio; therefore the problem of buckling exists and is recognized, and the authors' contribution to this little investigated subject is welcome and sound.

The testing arrangements described in the paper, to simulate idealized conditions, are so good that it is perhaps ungenerous to suggest that the idealized support conditions as indicated in Fig. 6 and as shown in standard text books, do not in fact provide perfect freedom for end rotation about a vertical axis. If the two rollers are pinned in position, the result is even worse than the arrangement shown, but Fig. A reveals that unless slip takes place between the rollers and the plane surfaces, a lack of fit $= \phi/2\ (1-\cos\alpha)$ occurs, where α is the angle of rotation about the vertical axis and ϕ is the diameter of the roller. This can be completely eliminated by the use of support conditions indicated in Fig. B, used by the present writer in his tests. It is only fair to add that it is not claimed that this matter has influenced the results of the authors' tests.

On p. 201, it is stated that the web reinforcement "undoubtedly contributed to the torsional strength of the beams and may have prevented potential torsional failures in those beams which developed large lateral deflections." While this is doubtful, it is nevertheless true that the web reinforcement provided, especially in the 6-ft span tests, was just sufficient to prevent shear deterioration, i.e., reduction of flexural capacity

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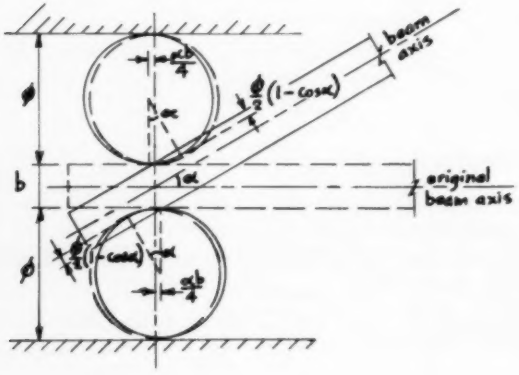
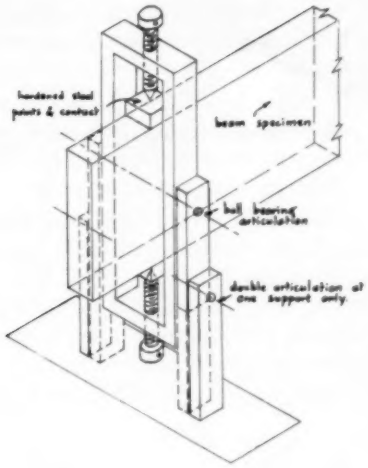


Fig. A — Diagram showing effect of slippage on the idealized conditions of hinged support

Fig. B—Diagram of beam supports. This theoretically gives an idealized support condition.



due to shear, as revealed by application of Subbiah's equation[19] to this case (F=1.01 for the 6-ft span test).

The use of the secant modulus of elasticity leading to Eq. (6) is examined below in relation to the writer's test evidence and appears to be well justified. It is a convenient approach to check whether a beam is likely to buckle below its flexural capacity (which is the only thing to matter in practical design) but if it is desired to calculate the actual buckling load, it is not so convenient, since it would be necessary to assume the buckling load to determine E_{sec}, calculate M_{cr} from Eq. (6), check E_{sec} and so on by successive approximation. The authors have well summed up the complexity of this problem in their remarks on pp. 204-205.

It is noted that the beams described in the paper contained 1.56 percent of main reinforcement. By Eq (3) of the ACI-ASCE committee on ultimate strength design,[20] the maximum percentage recommended (to secure tension failure) would be 3.9 percent. This would give a moment of resistance by Eq. (2a) of 413 in.-kip, which is considerably in excess of the figure of 328 in.-kip given by the authors from their Eq. (6) for buckling at $L/b = 32$. Hence it is not possible to agree with the authors' remarks on the ACI Building Code on pp. 207-208, since it would appear that the important factor influencing buckling is whether the beam has sufficient flexural capacity to achieve the buckling load, and this is evidently well within present design possibilities, provided sufficient web reinforcement is also provided.

The writer[21] tested 12 small reinforced micro-concrete beams, with idealized test conditions and center point gravity loading applied through the shear center which all failed by buckling, on 71 in. span.

(a) A series of 7 beams (plus one specimen restrained against lateral buckling, which failed at ultimate flexural capacity at 650 lb maximum load):

Section: $b = 0.675$ in., depth over-all 3 in. ($d = 2.62$ in.)

Main steel 0.067 sq in., compression steel 0.0275 sq in. located 0.2 in. below the top. Web reinforcement 0.74 percent.

Cube strength 4290 to 5620 psi.

E_{sec} using the curve of Fig. 9, and k had to be calculated appropriate to the beam condition at buckling, not ultimate flexural, load. For an average value of this buckling load = 296 lb it was found that neutral axis factor $k = 0.51$ and $E_{sec} = 2.75 \times 10^6$ psi satisfied the conditions of equilibrium and compatibility with the steel strain diagram, by a trial and error process. Then by Eq. (6) $M_{cr} = 4.46$ in.-kip.

The actual values of buckling moments were:

Specimen	1	2	3	4	5	6	7
M_{test}, in.-kips	5 33	5 57	4.08	3.91	5.5	5.85	6.55

The low values of Specimens 3 and 4 (of which the former was subjected to a cycle of restrained load greater than buckling value before its buckling test) are possibly due to some slight imperfections in the specimens whose nature is not known to the writer.

(b) A series of five beams:

Section: $b = 0.485$ in., depth over-all 4 in. $d = 3.52$ in.)

Main steel 0.067 sq in., compression steel 0.0275 sq in. (Specimens 1 and 2) and 0.0122 sq in. (Specimens 3 to 5) located 0.3 in. below the top. Web reinforcement 1.04 percent.

Cube strength 4440 to 6900 psi.

By similar calculation it was found that at average buckling load

Specimens 1 and 2, $k = 0.5$, $E_{sec} = 3.3 \times 10^6$ psi.

Specimens 3 to 5, $k = 0.53$, $E_{sec} = 3.2 \times 10^6$ psi.

Hence by Eq. (6), $M_{cr} = 2.88$ in.-kip for all specimens. Test results were:

Specimen	1	2	3	4	5
M_{test}, in.-kips	4.49	3.72	4.99	4.44	4.95

To provide a method more suitable for design use and avoiding the calculation of E_{sec}, it is suggested that Eq. (1) be written in the form

$$M_{cr} = \beta \, Y_{cr} \left(\frac{b^3 d}{L} \right)$$

for the elastic case, where β is the loading factor whose values are π for

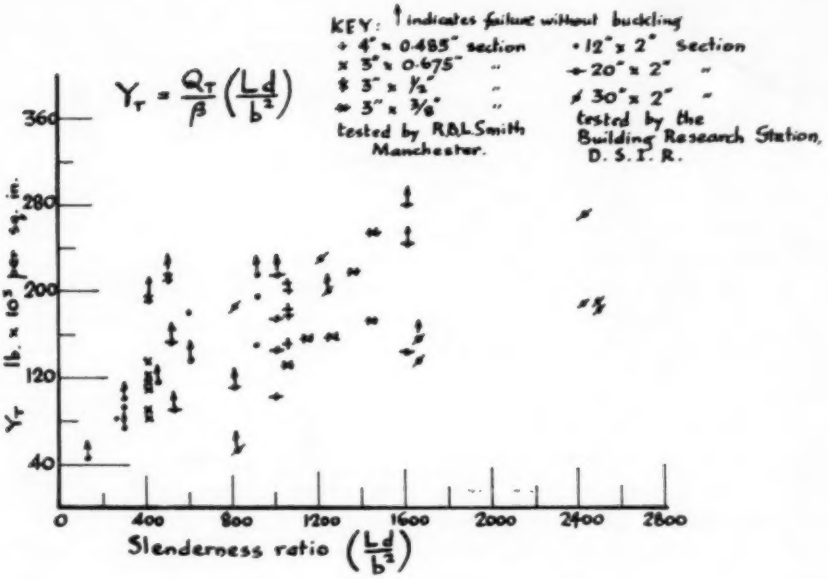


Fig. C—Slenderness ratio versus yield stress

constant bending moment, 3.54 for uniformly distributed load, and 4.23 for center point loading. The geometrical factor influencing buckling is $(b^3d)/L$, and $Y_{cr} = \sqrt{EG}/6$.

Then for the inelastic case, particularly reinforced concrete beams, let us write

$$M_{uc} = \beta\, Y_{uc}\left(\frac{b^3d}{L}\right) \qquad (8)$$

where M_{uc} is the actual buckling load predicted.

Y_{uc} is now an empirical factor, less than Y_{cr}, determined as the lower boundary of all test results of buckling failures. Such results, plotted* in Fig C indicate that although Y_{uc} is subject to heavy scatter for the reasons described by the authors, its lowest values are associated with smaller values of Ld/b^2; that is, those beams which buckled at loads approaching the ultimate in flexure, for which the effects of plasticity and cracking were greatest, corresponding to a low value of E_{sec} in Eq. (6). Thus while Eq. (8) will tend to underestimate the buckling loads of extremely slender beams, which fail at low values of M_{test}/M_u, it will be suitable for more practically important cases. There is a close analogy between this suggestion and the interaction of stability and plasticity in rigid steel frames.[22]

*Fig. C and D include previously unpublished data kindly provided by the Building Research Station in England.

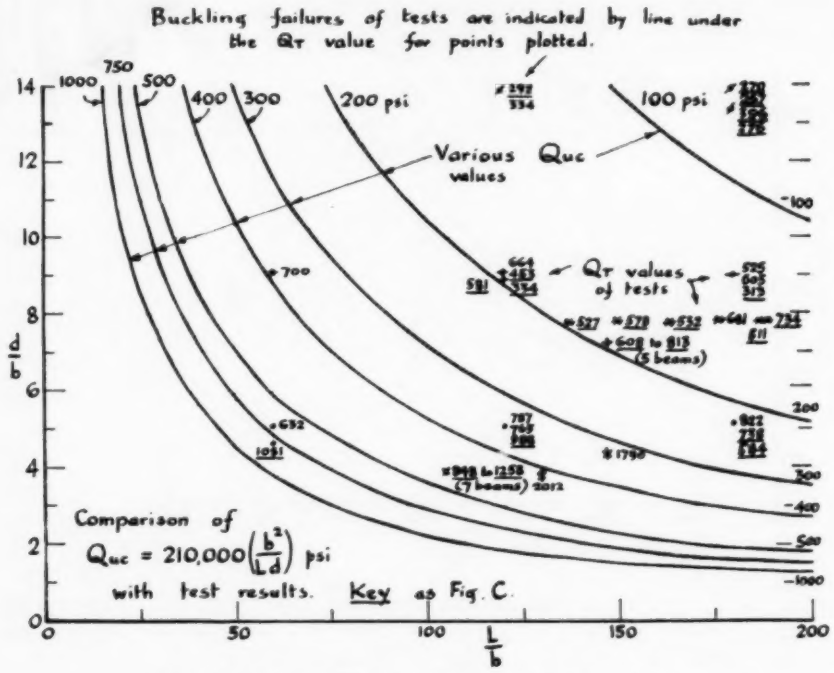


Fig. D—Values of Q_{uc} for any ratios of d/b and L/b

The test evidence so far available indicates that a value of $Y_{uc} = 60 \times 10^3$ psi might be adopted tentatively. This may be converted into a more useful form as follows, taking $\beta = 3.5$:

$$M_{uc} = 3.5 \times 60 \times 10^3 \left(\frac{b^2 d}{L}\right) \text{ in.-lb.}$$

and

$$Q_{uc} = \frac{M_{uc}}{bd^2} = 210{,}000 \left(\frac{b^2}{Ld}\right) \text{ psi}$$

gives a *moment of resistance factor* for buckling failures. Fig. D enables Q_{uc} to be determined for any ratios of d/b and L/b and in design it would only be necessary to ensure that the flexural moment of resistance factor did not exceed this, if necessary increasing b or providing lateral restraint.

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AUTHORS' CLOSURE

The authors are gratified to note the interest in the lateral stability problem as evidenced by the preceding contributions. These discussions, apart from pinpointing minor areas of misunderstanding, supply important supplementary information.

Mr. Bumann's criticism of the test arrangement seems to stem from a misreading of Fig. 4, or from a misunderstanding of what a beam does when it bends (buckles) laterally. It does so not by independent motions of top and bottom, as Mr. Bumann implies, but as a unit without any distortion of the cross section. That is, since the beam is one solid unit, the "top" is joined to the "bottom" by the body of the beam much more solidly and rigidly than any light flexible instrumentation yoke could ever do. If, on Fig. 1, such a yoke were attached to the dashed cross section at midspan, when the beam bends sideways as shown, the yoke would simply ride with the section into the shown, deflected position, without in the least affecting the behavior of the beam. The sole purpose of these light yokes was to hold the load and leveling plates immovably to the top surface of the beam. They could in no way affect the test results.

Regarding Professor Marshall's three points: (1) The difference in buckling moments for quarter-point loading and for pure bending is negligible, as we have pointed out indirectly in the paper, and as Professor Marshall himself indicates. The ratio of buckling moments for quarter point loads to those for pure bending depends somewhat on the vertical distance from the beam axis to the level of load applications, but does not deviate from 1.0 by more than a few percent. (2) As Professor Marshall himself says, the Mitchell correction (which reflects the influence of vertical bending deflections on lateral buckling loads) is entirely negligible for slender beams while beams which are not slender do not buckle. Therefore it is always negligible for actual beams slender enough to be liable to buckle. In the case of the test beams, I_x would have to be the moment of inertia of the entire transformed section, including the transformed area of the steel, so that the Mitchell correction $(I_x - I_y)/I_x$ is very close to unity and can be omitted. (3) The authors, of course, were far from "suprised" that buckling did not take place in their tests. In fact, one of them had tried to convince the pertinent ACI committee to relax the L/b restrictions in the Code purely on analytical grounds. The fact that the committee would not be convinced suggested these tests as proof of the analytical assertion that no buckling was likely to happen.

Professor Marshall's approximate buckling calculations must be evaluated with caution. Although approximately correct for the beams tested by the authors, his value $E = 2.5 \times 10^3$ ksi may lead to unsafe conclusions for beams involving intermediate to heavy steel ratios or high strength steels. When these conditions prevail, the plastic properties of concrete may reduce the modulus which governs stability (the secant modulus according to the author's analytical reasoning) to some 40 percent of Professor Marshall's value. The authors' proposed buckling relation, Eq. (6), accounts for the effects of the reinforcement ratio and the steel yield strength.

Concerning Mr. Smith's comments on the support conditions, it is noted that (1) the vertical rollers were not pinned in position and (2) slippage between the vertical rollers and the lateral support wings occurred in several of the tests. This relative movement which did permit beam rotation at the ends, amounted to small fractions of an inch and was evidenced by smear marks on the well greased surfaces of the lateral support wings. The loose fit of these supporting devices could, therefore, not produce restraint of any significant amount.

Mr. Smith correctly indicates that the minimum slenderness ratio at which lateral buckling can occur prior to flexural failure decreases as the tension reinforcement ratio increases. However, Mr. Smith's contention that the test beams would have buckled at $L/b = 32$ had the reinforcement ratio been increased to the maximum of 3.9 percent is not really correct. Such an increase in p would also increase k with the result that the conservatively computed buckling moment for $L/b = 32$ would exceed the flexural capacity of the test beams. Thus no buckling would occur prior to yielding of the tension steel.

The results of tests on small model beams presented by Mr. Smith supply important additional evidence on lateral buckling of reinforced concrete beams. If evaluated by the authors' buckling analysis, the following obtains: The average value of the ratio M_{test}/M_{cr} for Mr. Smith's 12 beams, is 1.37 which is quite close to the ratio of buckling moments for midspan loading (used in Mr. Smith's tests) to those for pure bending as used by the authors ($4.23/\pi = 1.35$). In all but two cases M_{test}/M_{cr} exceeds 1.18. This seems to indicate that the authors' approximate buckling analysis, which is based on the simple expression for pure bending, is reasonably reliable and conservative for beams under transverse loading.

It is pertinent to note that of the several dozen beam tests plotted in Mr. Smith's Fig. C and D, only one buckling failure is noted for L/b less than 100. This beam (2 in. $\times$ 12 in. with $L/b = 60$) buckled at $M/bd^2 = 1051$, indicating a very large reinforcement ratio. Depending on concrete strength this beam must have developed between 70 and 100 percent of its limiting ultimate flexural capacity as governed by compression. The

authors wonder whether this one apparent buckling failure may not have been initiated by yielding of the tension steel. However this may be, Mr. Smith as well as the investigators at the Building Research Station were apparently just as unable as the authors to obtain buckling failures at L/b ratios below 80 to 100 (with the one noted exception), in spite of their very high reinforcement ratios and large d/b ratios.

Mr. Smith's buckling relation, Eq. (8), is appealing in its simplicity. The authors concur with Mr. Smith that his empirical factor Y_{uc} is not really a constant. Reasoning from their Eq. (6), they suggest that it varies significantly with concrete strength and steel yield strength. Inspection of Mr. Smith's Fig. C shows that the lowest experimental value was $Y_{uc} = 84 \times 10^3$ psi, with a mean value of the order of 160×10^3 psi. Hence, Mr. Smith's proposed value of 60×10^3 psi seems to be too much of a simplification, and excessively conservative even in the region of equal flexural and buckling strength.

Two additional items of interest are apparent from Mr. Smith's Fig. D, even though this figure also seems excessively conservative since it is based on $Y_{uc} = 60 \times 10^3$ psi. (1) Values of $Q_{uc} = M/bd^2$ much in excess of 1000 (the highest value in Fig. D) are improbable even with very high steel ratios, except when high strength reinforcement is used. Fig. D shows that by Mr. Smith's analysis, even for this high value, buckling at $L/b = 32$ would require a d/b ratio as high as 7 or more, an unrealistic situation which confirms the excessive restrictiveness of the $L/b = 32$ limit. (2) The M/bd^2 ratio for the beams tested by the authors was 620 and for their $d/b = 4.5$, Mr. Smith's Fig. D would predict buckling at $L/b = 75$. However, the test results showed that no buckling had occurred for L/b as high as 86. This confirms that Fig. D, as surmised above, is very conservative and that buckling actually takes place at even larger L/b values. This is also borne out by Mr. Smith's own tests and by those of the Building Research Station, as plotted in that figure.

The authors would like to thank the discussers for their thoughtful comments. Their contributions supplement the authors' paper and together with it provide a much better understanding of the lateral stability problem than was previously available.

Discussion of a paper by Henry Miller:

Chicago's 39-Story R/C Executive House*

By FRANK RANDALL, IRVING B. RAU, and AUTHOR

By FRANK RANDALL†

Tall building construction with reinforced concrete frames has had a great upsurge in recent years, and Mr. Miller is to be thanked for his article disclosing the details of the tallest concrete frame building in the United States. Increased height with concrete structures is now possible because of several factors which minimize column sizes, for example, high strength concrete, high yield point reinforcing bars, larger bar sizes, and the ultimate strength design method. Also, shear walls are becoming increasingly popular to resist lateral forces.

The article states that 110,000 psi ultimate strength steel was used with only a 20,000 psi working stress due to the local building code, and that this gives a high safety factor. It would also be worthwhile to state the matter a different way, i.e., on the basis that a higher working stress could be allowed, greater loadings are conceivably possible. This is particularly pertinent to wind or lateral loading, a prime matter in tall building design. In other words, this building could possibly withstand, safely, higher wind loads than the 21.2 psf used in the design.

The Executive House not only has the distinction of being the tallest of its kind in the country, but to my knowledge is the first concrete building in the United States to break the height record set in 1927. That was a height of 299 ft set by the 20-story Master Printers Building at 10th Avenue and 34th Street in New York City. The 24-story Illuminating Building in Cleveland was constructed to a height of 298 ft in 1957 and almost toppled that long-standing record. The second highest concrete structure in the United States is an apartment building just recently topped-out in Chicago; the 1550 Lake Shore Drive Building is 35 stories above grade plus a two-story penthouse, or 339 ft from grade to the top of the penthouse framing.

While the article states that the Executive House is the fifth tallest concrete framed building in the world, a new compilation rates it as the tenth tallest. Construction of tall concrete structures is more widespread than is generally supposed. The list is as follows:

*ACI JOURNAL, V. 31, No. 3, Sept. 1959 (*Proceedings* V. 56), p. 215. Disc. 56-15 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).

†Member American Concrete Institute, Structural Engineer, Portland Cement Association, District Office, Chicago, Ill.

Rank	Height, ft	Stories	Name and location	Reference
1	507	34	Banco do Estado de Sao Paulo, Sao Paulo, Brazil	1
2	443	45	Italia Building, Sao Paulo, Brazil	2
3	414	30	Pirelli Building, Milan, Italy	3
4	410	42	Atlas Building, Buenos Aires, Argentina	4
5	406	35	La Torre de Madrid, Madrid, Spain	5
6	402	36	Edificio Focsa, Havana, Cuba	6
7	385	29	Edificio Espana, Madrid, Spain	7
8	384	31	Grattacielo del 'orologio, Genoa, Italy	8
9	381	32	Grattacielo di Milano, Milan, Italy	9
10	371	38	Executive House, Chicago, Ill.	10

Several other buildings under construction in Argentina and Venezuela may also now belong in the above list.

As a uniformity of measure, the height (in feet or meters) and number of stories above grade are suggested. The height in stories would exclude penthouses and staggered stair landings, but would include mezzanines and loaded attics which cover the full area of the building. The height of a building by stories is often confused by specialized numbering and lettering systems in foreign countries where the "first" floor is actually the fourth by our standards; in this country the 13th floor is frequently omitted in the numbering. For comparative purposes, it is suggested that height in feet be measured from grade to top of penthouse framing, excluding parapets, flagpoles, and television antennas unless specially mentioned.

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10. *Architectural Forum*, Aug. 1959, p. 124.

By IRVING B. RAU*

Mr. Miller is to be congratulated for his design accomplishment on the 39-story Executive House in Chicago.

He states that the design wind pressure was computed to be 21.2 psf which was used for the full height. This must conform to the Chicago

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building code, I assume, inasmuch as I do not have knowledge of the Chicago code.

Calculating on one shear wall at 40.5 ft centers gives a total horizontal force of 318 kips and a total moment of 59,000 ft-kips, see Fig. A. These values will be used as the base for the comments that follow.

If this building had been located just outside the limits of Chicago where the city building code is not required to be used, then the designer is at liberty to design this structure with whatever code he desires. If the designer looks around he can find many national codes to guide him. One of these codes is the "National Building Code" recommended by the National Board of Fire Underwriters, 1955 Edition.

Fig. A—Wind forces

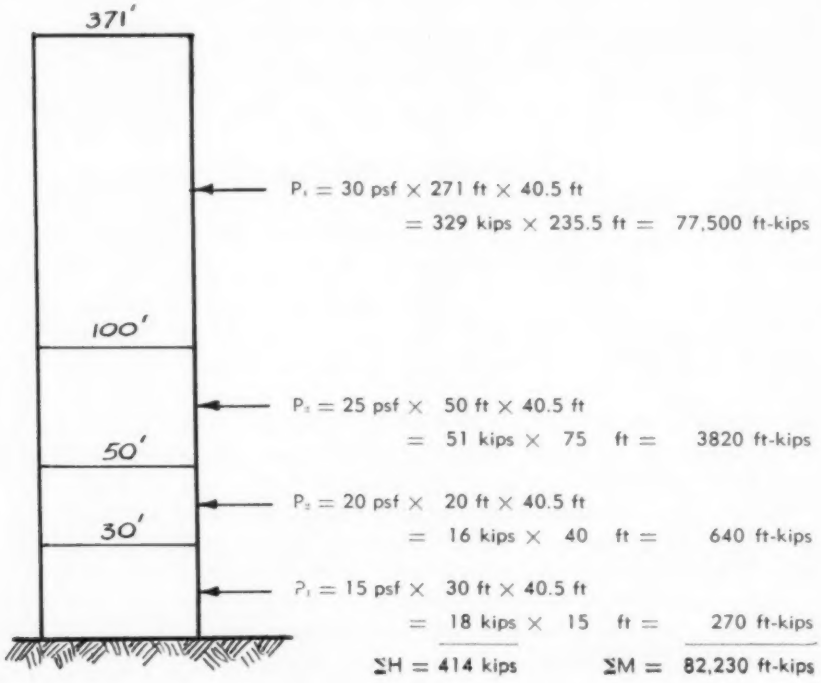
From p. 219, ACI JOURNAL, Sept. 1959:

Shear walls at 40.5-ft centers

Wind force = 21.2 psf × 371 ft × 40.5 ft

$$= 318 \text{ kips} \times \frac{371 \text{ ft}}{2} = 59{,}000 \text{ ft-kips}$$

From "National Building Code," recommended by the National Board of Fire Underwriters, 1955, Section 903:


371'
100'
50'
30'
P1 = 30 psf × 271 ft × 40.5 ft
= 329 kips × 235.5 ft = 77,500 ft-kips
P2 = 25 psf × 50 ft × 40.5 ft
= 51 kips × 75 ft = 3820 ft-kips
P3 = 20 psf × 20 ft × 40.5 ft
= 16 kips × 40 ft = 640 ft-kips
P4 = 15 psf × 30 ft × 40.5 ft
= 18 kips × 15 ft = 270 ft-kips
ΣH = 414 kips
ΣM = 82,230 ft-kips

Section 903 of this code gives design wind pressures. These are shown on Fig. A. Results using these wind pressures are a total horizontal force of 414 kips (30 percent above base) and a total moment of 82,230 ft kips (39 percent above base). This is quite a variation, but not one of too much concern considering the unknowns in design wind pressures.

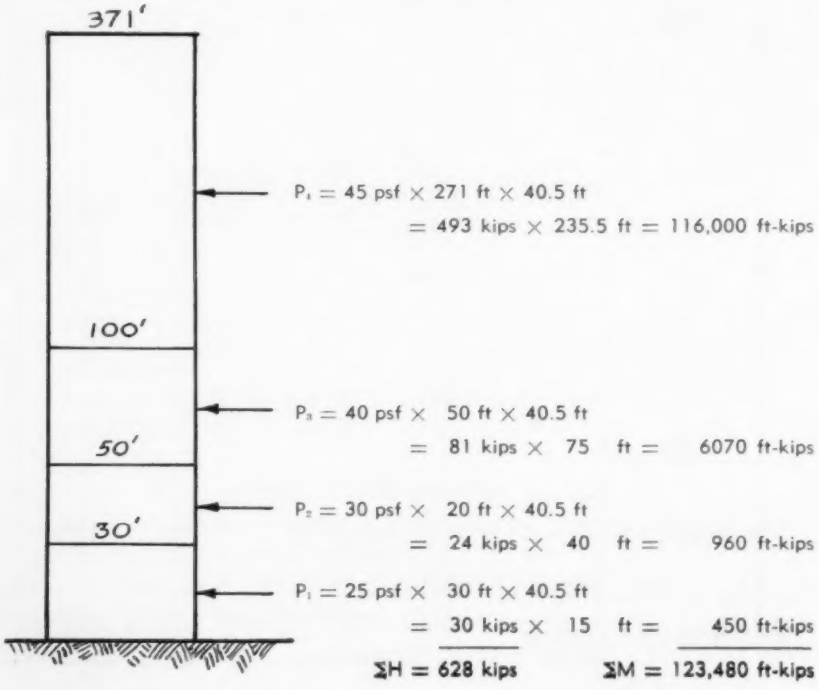
Now let us look at another national code, the "Minimum Design Loads in Buildings and Other Structures" of the American Standards Association (Sept. 1955 edition).

Wind pressures from this code are shown on Fig. B. Results using these wind pressures are a total horizontal force of 628 kips (97 percent above base) and a total moment of 123,480 ft-kips (109 percent above base). This variation is quite large.

The wind pressures presented in the code of the American Standards Association are supposed to be one of the latest compilation of results available today, and many designers are using these recommendations

Fig. B—Wind forces

From "Minimum Design Loads in Buildings and Other Structures," American Standards Association, Sept. 1955, Chicago, Ill., is in 30 psf wind-pressure-map area.


371'
100'
50'
30'
P1 = 45 psf × 271 ft × 40.5 ft
= 493 kips × 235.5 ft = 116,000 ft-kips
P3 = 40 psf × 50 ft × 40.5 ft
= 81 kips × 75 ft = 6070 ft-kips
P2 = 30 psf × 20 ft × 40.5 ft
= 24 kips × 40 ft = 960 ft-kips
P1 = 25 psf × 30 ft × 40.5 ft
= 30 kips × 15 ft = 450 ft-kips
ΣH = 628 kips
ΣM = 123,480 ft-kips

in designing their frames. However, it is known that many older buildings have been built and have performed satisfactorily using wind pressures less than those now recommended.

Many of these older buildings have masonry walls and partitions which contribute to the wind bracing. The Executive House does not have masonry walls, but is of modern design using curtain walls and depending upon the structural frame to resist the wind forces.

These wind forces can become quite a factor in the design of tall buildings. In areas where the wind is known to reach sizable velocities buildings are designed for these large forces. There are areas where the wind velocities are not expected to reach large proportions and local codes call for small wind pressures. However, these areas are occasionally subjected to wind velocities larger than those normally expected, as for example the hurricanes that have been hitting the East Coast the last few years.

Structures in these areas were designed with small wind pressures and there have not been any published reports brought to the writer's attention of major buildings reported to have shown distress.

The writer wishes to point out this contradiction and to admit that structures have been designed for smaller wind pressures and have performed with no distress. However, the recent investigations and reports tend to show that wind pressures can be larger than was expected years ago. In view of the many variables and unknowns in wind pressures, and with modern architectural design using skeleton frames and curtain walls, it is the writer's opinion that the designer should be prudent and use the larger wind pressures.

AUTHOR'S CLOSURE

Mr. Rau's criticism of the 21.2 psf wind load used in the design of the 39-story Executive House is appreciated. The article stated that this wind load was used for the full height of the building.

The Chicago building code requires that all buildings up to 300 ft in height be designed for a wind load of 20 psf. For any height above 300 ft the wind load shall be increased by 0.025 psf for each foot of increase in height.

Thus to conform to the Chicago building code, the Executive House should have been designed for a wind load of 20 psf for the lower 300 ft and 21.8 psf for the upper 71 ft, or an average for an equivalent moment for the full height of 20.7 psf. However, because of the uncertainty of the height limit of the top story at the time of designing the building, an average of 0.5 psf was added over the whole building for a total of 21.2 psf.

Perhaps the trend in some parts of the country is toward higher wind loads, yet here in Chicago the trend of the required design wind load has

been lower for many years. Several tall buildings of structural steel frame meeting this code have been built in the last few years. None of these show distress.

The Joint Committee on Overhead Line Construction of the National Electric Light Association has studied wind pressures. They give the formula of $P = 0.0042V^2$ where P is the pressure in psf of a projected flat area, and V is the actual wind velocity in miles per hr. Thus, for a P of 20.1 the actual velocity equals 69.2 miles per hr. This according to the United States Weather Bureau is equivalent to a 90 miles per hr indicated wind velocity. This high a velocity, I believe, has not yet been recorded in the Chicago area.

But, in spite of the above, let us say that Mr. Rau is justified in assuming that a higher wind load should have been used. Let us assume the code with the highest wind loads is used. According to Mr. Rau the overturning moment is 123,480 ft-kips. This is far less than two-thirds of the stabilizing moment due to dead load only. Although anchoring of the building was not required, it is, nevertheless, anchored to the foundation girders and on down to the caissons.

As for the internal stresses due to wind loads, this structure can withstand several times the actual wind loads. The main reason for this is the fact that a high ultimate strength steel (110,000 psi) was used with an allowable of only 20,000 psi. The Chicago building code allows a 33⅓ percent increase in design stresses when considering wind loads in combination with dead and live loads.

In using the wind loads as required by the Chicago building code, the designers feel they were prudent, especially in view of the fact that the allowable working stresses are much too low for the materials used.

Perhaps what Mr. Rau wishes to suggest is that the Chicago code requirement in this respect be revised upward.

Referring to the discussion by Mr. Randall, I wish to extend many thanks to him for his comments.

One thought could be added to Mr. Randall's discussion. I refer to the fact that the Executive House has created a great deal of interest in designing tall buildings with reinforced concrete frames in the United States. It also brought to the front the lack and need of mathematical analysis of shear wall design.

From data available to me at the time the original article was written, Executive House was the fifth tallest reinforced concrete building in the world. However, Mr. Randall's research in the matter places this building in the tenth position. I believe that Mr. Randall should be commended for pointing out the facts.

After reading Mr. Rau's discussion regarding our use of 21.2 psf wind load, it is interesting to note what Mr. Randall says in his second paragraph.

Discussion of a paper by Martin J. Gutzwiller and Joseph L. Waling:

Laboratory Study of Pavements Continuously Reinforced with Deformed Bars*

By BENGT F. FRIBERG and AUTHORS

By BENGT F. FRIBERG†

The authors have related *all* developments in these interesting laboratory tests to simulated temperature drops as shown in Fig. 7 to 19. Their intent was obviously to suggest that the experimental data are applicable to corresponding behaviors in concrete pavements. Such a method of presentation is not the easiest for objective study of the tests, nor is its correlation to conditions in continuously reinforced highway pavements substantiated by highway observations, probably because of the imposed experimental limitations.

The temperature drop simulated in the experiments is dependent largely on the number of cracks in the test region, three of them preformed and representative for pavement developments at exceptional, not average, conditions. The experimental procedure was notably different from the most probable mechanics of stress developments in pavements. Moisture from the subgrade has a greater effect than subgrade elastic properties on variations in crack width from top to bottom of the pavement; however, the desirability of equal crack widths at top and bottom is in itself entirely conjectural. Small vertical deflections, or movements, at cracks appear to be a desirable aim; however, on that score the experiments give no clear relationship to the primary experimental variable of steel percentage, nor would any deductions be valid without crack spacings more nearly representative for highway conditions. Any application of the few experimental data to pavement design criteria is accordingly questionable.

No criticism is implied of experimental aim to verify developments in continuously reinforced pavements in the laboratory. Adequate information is believed to be possible, provided procedures were varied sufficiently to duplicate different probable crack inducing developments in highways. This is seen as the primary objective of the investigation. Only one experimental procedure was used and not one very illustrative of actual occurrence in pavements.

*ACI JOURNAL, V. 31, No. 3, Sept. 1959 (*Proceedings* V. 56), p. 223. Disc. 56-16 is a part of copyrighted JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, V. 31, No. 9, Mar. 1960 (*Proceedings* V. 56).

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Temperature and shrinkage

The simulated temperature drop t is apparently related to the observed change in length of the 130 in. "central test region," containing three preformed cracks spaced 5 ft. apart. Comparisons between observed changes in crack widths, Fig. 11 and 12, and the crack widths which would be present if all length change occurred at the three cracks, $0.000006 \times t \times 130/3$, indicates that the crack openings were about as wide as they would be for unrestrained temperature contraction at the cracks. The experimental data then, apply substantially to 43⅓ in. average crack spacing with respect to comparative pavement temperature drop, rather than 60 in. as might be inferred. It is regrettable that no information has been given on amount of strain between the cracks; however, the data appear to refute suggestions that steel reinforcement as used in the tests can keep cracks from opening substantially unrestrained amounts indicated by temperature changes. If the central test region had been made 180 in., substantially lower abscissa values of simulated temperature drop would have applied, and for some of the tests closer to field correlations.

Certain crack widths are indicated in Fig. 11 and 12 for zero temperature drop. It is assumed that those cracks were observed prior to any vertical loading, and occurred during insertion of the elastic subgrade, thus they would correspond to shrinkage cracks in pavements. Those intial crack widths have not been plotted for equal simulated temperature drop, although in fully restrained pavements shrinkage they would be analogous to temperature drop both as regards concrete stresses and steel stresses (see *Proceedings*, ASCE, V. 85, HW 2, Apr. 1959, p. 16). The total elongation for longitudinal force, including the initial crack widths, has apparently been used in determining the other than zero simulated temperature drops, which therefore represents the combined effect of shrinkage and temperature drop. Steel stresses at cracks would not be zero in restrained pavements for the crack widths indicated at zero temperature drop. Steel stresses plotted in Fig. 16 and 17, therefore, are in addition to steel stresses at cracks due to shrinkage, although plotted in relation to simulated temperature drop including shrinkage. The shape of all curves for small temperature drops is accordingly not exact. If the initial crack widths were expressed as equivalent temperature drops based on temperature drop for next higher longitudinal force increment, and if steel stress at cracks for the first longitudinal force were extrapolated, the following temperature equivalent of initial crack width, and corresponding steel stress at cracks due to such shrinkage, are computed:

0.28 percent, initial crack 0.003 in., 9 F equivalent drop, 7,000 psi steel stress
0.43 percent, initial crack 0.005 in., 14 F equivalent drop, 13,000 psi steel stress
0.54 percent, initial crack 0.002 in., 5 F equivalent drop, 6,000 psi steel stress
0.76* percent, initial crack 0.004 in., 12 F equivalent drop, 6,000 psi steel stress

*Equivalent temperature drop for three cracks, steel stress for four cracks in test region.

Steel stresses, plotted in relation to simulated temperature drop which includes shrinkage, would be higher than the amounts shown in Fig. 16 by up to the above listed values.

The vertical loading sequence apparently exerted a major effect on cracking and a cumulative influence on crack widths. Presumably, three vertical loads, 5000, 10,000, and 15,000 lb were applied at one load point, removing the load after each magnitude, and switching from 12- to 18-in. loaded circle between the 5000- and the 10,000-lb loads; the above sequence was repeated at each of eight load points, for a total of 24 separate load applications prior to and at each longitudinal force. With respect to the longitudinal force simulating temperature restraint, on the other hand, apparently the force was not removed between each series of 24 vertical loadings, but was increased progressively by 10,000-lb increments, and was not permitted to decrease during the vertical loading. The experiment accordingly involves no repetition of contraction and expansion at cracks, such as would occur in pavements from early age, before any application of wheel loads.

All abscissa values of simulated temperature drop are derived from crack-width and strain measurements at the predetermined longitudinal force, but before vertical loading sequence. Fig. 18 compared to Fig. 16 shows stress increases averaging 17,000, 5000, 10,000, and 6000 psi in Slabs 1 to 4 for the maximum vertical loads, and increases in average crack widths (Fig. 14) on the order of 0.010 in. while 15-kip loads were in place over the cracks. (Longitudinal force being constant, a couple existed at the preformed cracks under vertical load, friction under the loading plates providing the compression force.) It is doubtful that such crack width and tension stress increases could occur in pavements where flexural stresses at open cracks are less certain and deflections are concentrated under the wheels with small over-all effect on crack width, except as a cumulative effect. (No such field observations have ever been made to this discussor's knowledge.) The experimental crack width increase under vertical load probably was accompanied by increase in active bonding length, and in that case undoubtedly was part irreversible. The simulated temperature values then were not fixed; they would increase during the vertical load sequence. (In pavements the restraint forces in the steel more probably would decrease cumulatively because of progressive increase in active steel bonding length in pavements subject to wheel load repetitions and temperature reversals.)

Top and bottom crack widths

Crack widths on the sides of the slabs at the level of the steel were apparently measured only on Slabs 6 and 5 with the steel 1 in. and 2 in. below center, respectively, and no measurements were made at bottom. The

authors have assumed (Fig. 13) that constriction of the crack occurred from the surface to the level of the steel, although the bars were nearly as far away from the sides, and that the cracks were wider at the bottom, with proportionate constriction also from the bottom to the level of the steel. Any such effect is believed to have been totally without significance. The major reason for cracks being wider at the top than further down is most certainly slab curvature, deflecting the slab elements into dished contour, with the cracks narrowing all the way from top to bottom. Because of drying from the top predominantly, dishing might have been observed also in the slabs with the steel centered.

Curvature of slabs with eccentric steel is an elemental effect of the eccentric force application, which is aggravated by drying of the slabs from the top. (It is unfortunate that side measurements were not made on the slabs with steel at middepth so that the effects of differential shrinkage from top to bottom could have been compared with effects of eccentricity.) The eccentric longitudinal force was centered at the steel; the lengths of slab between cracks were insufficient for any appreciable restraint of curvature through moment caused by redistribution of subgrade support. The computed stresses in the composite bonded sections between cracks for a longitudinal force P and $n = 10$, are:

	Top stress	Bottom Stress
Slab 6, steel 1 in. below center	$+0.0008P$	$+0.0057P$
Slab 5, steel 2 in. below center	$-0.0016P$	$+0.0080P$

As seen, there is compression at the top surface between cracks of Slab 5 due to the longitudinal forces with corresponding rather substantial slab curvature. (For 50,000 lb longitudinal force the top stress would be about 80 psi compression, the bottom stress 400 psi tension, the radius of curvature about 50,000 in., the slope change in 60 in. 0.0012 radians, and the dishing about 0.009 in. Because of the curvature between and beyond the three preformed cracks they may have averaged about 0.01 in. wider at top than bottom without vertical load, even without intermediate cracks extending part way up in the slab.) It would be impossible to suggest an optimum level for the steel without taking into account differential shrinkage or swelling which was neglected in the experiments, yet is quite noticeable in the field. Warping, in turn, influences deflections which cannot be studied adequately without consideration of these variables.

Correlation with continuously reinforced highway pavements

Crack spacing—Proponents of continuous reinforcement consider the propagation of cracking most important, the steel being intended to cause additional cracks on decreasing temperature, rather than that existing cracks should open wider. It is pertinent to compare the experimental

slabs with highway pavements on the basis of actual observed crack frequencies. Values of cracks per 100 ft give a better idea than the minimum crack spacing of restraints by the steel at cracks to temperature drop, because in long restrained pavements the longitudinal force cannot vary appreciably from crack to crack, except as a result of subgrade frictional force increments to the forces in the steel, for which cumulative movements are necessary, and of any consequence only over a considerable number of crack intervals.

Additional cracks formed in most experimental slabs. The three preformed cracks probably were too close together in the slabs with the smallest steel percentages for critical combined direct and flexural tension stresses betwen the cracks, but that was apparently not the case in slabs reinforced with 0.54 and 0.76 percent steel. The number of cracks in the 130-in. central test region, and corresponding crack frequency within 100 F simulated temperature drop, were as follows:

Reinforcement, percent	Cracks in 130 in.	Temperature drop, F	Average crack spacing, ft	Cracks per 100 ft
0.28	4	47	2.7	37
0.43	3	to 100	3.6	28
0.54	6	70	2.0	50
0.76	6	87	1.8	54

Note: Crack spacing nearest outside the preformed cracks is considered in the frequency for 0.54 and 0.76 percent steel.

The number of cracks per 100 ft in the experimental slabs was approximately twice the *maximum* frequency observed in the Illinois 7-in. sections at 3 years age (*Bulletin* No. 214, Highway Research Board, 1959, p. 24), nearly twice the maximum in the California project with 0.5 and 0.62 percent steel at 1 year, and over twice the maximum on a Pennsylvania project near Hamburg with 0.5 percent steel at 1 year (*Proceedings,* Highway Research Board, V. 38, 1959). The longitudinal stress imposed by the steel at the cracks and the vertical loading were accordingly significantly more effective in causing additional cracks to form in the experimental slabs than the same amount of steel in highways for many repeated temperature cycles. The differences between the experimental slabs and highway performance deserve primary attention. They are believed to be the result, in a large degree, of the unrealistic loading conditions on and crack spacings in the experimental slabs.

Steel stress at cracks—Comparisons of steel stresses at preformed cracks in highway pavements and in the slabs are illuminating. On the Illinois project sections reinforced with 0.7 percent steel of ⅝-in. high-bond bars[15] concrete temperatures and steel stresses were observed: for the first several months in the 7-in. section placed in early October, with about 14 ft. average crack spacing at 1 month and 11 ft at ½ year;

through the first summer and fall in the 8-in. section placed in late April, with about 14 ft crack spacing at 8 months, and about 9 ft at 2 years. During the first 4 months of age the 7-in. section was exposed to seasonal temperature drop, the 8-in. section to summer temperatures; restraint tension may have predominated in the 7-in., and restraint compression in the 8-in. pavement. Traffic loads apparently started after stress observation on the 7-in., and during stress observations on the 8-in. section. For equal temperature drop the steel stress at cracks in the pavements with 14 ft crack spacing (about 1 month age), should be much greater than in the experimental slabs with three cracks in 130 in.; however, for a 20 F temperature drop near 1 month age, in the pavements the observed steel tension was only 22,000 psi in the 7-in. and 20,000 psi in the 8-in. section without any vertical traffic loads; and, by comparison, in the experimental slab with 0.54 percent steel of $\frac{5}{8}$-in. bars crack width increased 0.005 in. for 20 F drop (Fig. 11) and steel stress increased over 15,000 psi (including shrinkage, steel stress would approximate 21,000 psi). Comparison with the 0.76 percent slab cannot be made because of #6 bars. Crack spacing considered, in comparison with the experimental slab with the same size bars, the steel restraint stress in the pavements at 1 month age for 20 F temperature drop should have been much higher than observed; furthermore, in the pavements the temperature drop necessary for 20,000 psi restraint stress increased progressively, to between 40 and 50 F at the early season of cold temperatures. Creep in tension could have caused the progressive loss of restraint in the 7-in., but hardly in the 8-in. section which was in summer compression restraint during much of the intervening time; and no such creep is indicated by observed stresses during the following seasonal increase in temperature. The lower—and decreasing—restraint by the steel in the pavements can be explained readily only as progressive loss of bond near the cracks. Any such development was prevented in the experimental slabs, where the longitudinal force was applied only once and increased progressively. The experimental structure was adequate for investigation of this pertinent problem of longitudinal restraint variations.

For a full comparison between the experimental slabs and pavements it is pertinent to suggest why highway pavement crack spacing has not later become more nearly comparable to the experimental results, inasmuch as, for the much greater crack spacing, the observed stresses in the steel in pavements reached even higher magnitude than imposed in the experiments. The elastic subgrade under the slabs probably was representative for at least some stretches of pavements. Flexural tension stresses in the bottom of the slabs under 15 kip loads probably were more serious, because the maximum stress acted at nearly even intensity on the full slab width in one direction, whereas in pavements maximum stress is more limited in extent, if not also in magnitude, and the extreme com-

bined stresses in pavements can be relieved through redistribution over a greater pavement width, which was not possible in the experimental slabs, as tested. In the experimental slabs flexural warping restraint stresses could not become noticeable (less than 5 percent), because of the short initial distance between cracks; however, in pavements on subgrades flexural restraint stresses to warping reach substantial magnitude for crack spacing on the order of 10 ft or more, with tension near top and compression near bottom predominating. These warping restraint stresses counteract wheel load stresses between cracks to a great degree. The crack spacings were large in the pavements during the first year, when restraint tension reached significant magnitudes; during later years, with bar bond apparently decreasingly effective, presumably steel tension restraint stresses could no longer reach magnitudes to exert any significant crack propagating function, and, as a consequence, further small crack development has been due to other causes.

In the experimental slabs the maximum steel tension and cracks and concrete tension in bonded portions between cracks, based on longitudinal forces indicated in the graphs for the different slabs, and simulated temperature drops shown in Fig. 16, within seasonal probabilities, were:

0.28 percent, #4 bars,	50,000 psi steel,	140-psi concrete,	86 F plus, drop
0.43 percent, #5 bars,	40,000 psi steel,	170-psi concrete,	64 F plus, drop
	(48,000 psi steel,	200-psi concrete,	103 F plus, drop)
0.54 percent, #5 bars,	32,000 psi steel,	170-psi concrete,	70 F plus, drop
0.76 percent, #6 bars,	27,000 psi steel,	200-psi concrete,	87 F plus, drop

If it were possible to anticipate crack spacing in pavements at early age comparable to that used in the experimental slabs, steel stress over 40,000 psi in #5 bars, and somewhat less in #6 bars, apparently would not occur in such continuously reinforced pavements. These limits are indicated by bond slip at the cracks in the slabs. For longer crack spacings the same tension restraint stresses would correspond to lesser temperature drops, and stresses could reach higher magnitudes, perhaps sufficient to indicate the crack propagating stresses, in combination with warping and curling restraint stresses, at very early age in some pavements with higher steel percentages and laid at the highest seasonal temperatures.

Considering the rather important differences between the experiments and highway pavements, which have been enumerated in this discussion, it seems conjectural to apply the summary observations and suggested design criteria to highway pavements. The experimental laboratory approach is nevertheless commendable. If it is pursued with objective interest in learning about the behavior of continuously reinforced pavements—without limitation to unrealistic procedures and details of imaginary design pertinence—information of lasting value could follow the initial impressive results.

AUTHORS' CLOSURE

The authors are pleased that Mr. Friberg has studied and discussed the paper. In general, the discussion ably emphasizes the fact that some of the bothersome variables encountered in field tests were controlled in the reported laboratory experiments. Indeed, this was one of the main objectives of the laboratory research.

It is clearly understood that moisture content of slabs does affect the widths of cracks in the slabs. However, to state that "moisture from the subgrade has a greater effect than subgrade elastic properties on variations in crack width from top to bottom of the pavement" is to add to the vast store of quantitatively unsubstantiated opinion. As stated in the paper, "Changes in crack widths with passage of wheel loads have not been measured in field experiments."

When one considers the mechanisms by which silt and other deleterious substances enter cracks—washed in by water under gravity from above and forced in with water under pressure from below—the desirability of equalizing and minimizing "active" crack widths at the top and bottom of a slab does not seem entirely conjectural. Until someone offers something substantially better as a criterion, the authors will retain their belief in equalizing and minimizing top and bottom active crack widths.

Mr. Friberg questions the use of a central test region of 130 in. as the basis for determining the magnitude of simulated temperature drop. In the slabs adequately reinforced, additional cracks were formed and the 60 in. crack spacing was therefore not maintained. The use of 180 in. as a central test region would have changed the simulated temperature drop only slightly since additional cracks also formed outside of the 130 in. central test region.

In his discussion of temperatures and shrinkage, Mr. Friberg *assumed* that the preformed "cracks were observed prior to any vertical loading." In general, this is not the case and his subsequent discussion concerning crack widths and steel stresses does not seem pertinent. Furthermore, the reader's attention is called to Item 2 of the loading procedure in which it is seen that the horizontal load was removed along with vertical loads before the next higher horizontal load was applied. Thus, the experiments did involve repetition of contraction and expansion.

The discusser apparently misinterpreted the results shown on Fig. 14. The increase in upper surface crack widths due to 15-kip wheel loads as shown on this figure occurred in every case when the 15-kip load was placed over *adjacent* cracks. Never would the top surface crack width increase due to a load placed directly overhead. Furthermore, the rubber padding between the bearing plates and the slabs never deterred cracks from closing when loads were placed directly overhead.

It is to be remembered that a single wheel load, while tending to decrease the width of a crack directly underneath, tends to increase the widths of adjacent cracks. Thus, simulated temperature values obtained during vertical loading would not substantially differ from those obtained for the slabs subjected only to horizontal loads.

The discusser called attention to specific differences existing between the laboratory data on average crack spacing and steel stress at cracks and that reported for two of the several field tests. Exact correlations cannot be expected to exist, due primarily to the great differences between the subgrade modulus of laboratory slabs and that of the field pavements. The 160 lb per cu in. subgrade modulus of the laboratory slabs represents a fairly flexible subgrade for which the effect of vertical wheel loads would be quite pronounced. Most, if not all, of the field pavements rested on considerably stiffer subgrades. It is, therefore, not surprising that a close correlation between laboratory and field data is not indicated. Certainly the differences that exist are in the right direction, both from the viewpoint of differences in subgrade modulus, as well as the difficulties of observing hairline and partial cracks and accurately measuring stresses in the field. In the continuation of laboratory experiments, slabs are being supported on stiffer subgrades so that the effects of the variable of subgrade stiffness can be studied. Likewise, the variable of slab thickness is to be included.